



**THE
NINTH
LACUS
FORUM
1982**



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1982**

**edited by
John Morreall**



HORNBEAM PRESS, INCORPORATED
6520 COURTWOOD DRIVE • COLUMBIA, SOUTH CAROLINA 29206

**THE NINTH
LACUS FORUM
1982**

Edenda curant

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FIRST EDITION

Published in Columbia, South Carolina by Hornbeam Press, Incorporated,
6520 Courtwood Drive, Columbia, S.C. 29206.

Manufactured in the United States of America.

ISBN 0-917496-23-X

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INTRODUCTION

For its Ninth Forum, held August 2-6, 1982, the Linguistic Association of Canada and the United States returned to the Chicago area, where the First LACUS Forum was held eight years ago. This time the site was the Evanston campus of Northwestern University. The Great Lakes have always been the center of gravity of LACUS — for this Forum we had Lake Michigan right outside the window of the meeting room. In a full schedule of papers and evening lectures, Forum participants had to make time for sightseeing and socializing. Northwestern's Linguistics Department helped out here by hosting a reception for LACUS and the TESOL Summer Institute being held at Northwestern.

As in past years the Ninth Forum drew participants from all over Canada and the United States. But this year also saw an increase in participants from Europe, specifically from Poland, Sweden, Germany, France, and Great Britain. Might LACUS be on its way to becoming LACUSE?

The traditional diversity of interests and methodological diversity of LACUS members was in full evidence this year too. Choosing headings under which to group the Forum papers is always a somewhat Procrustean affair; I have tried to keep the arbitrariness to a minimum by at least using familiar standard headings.

Evanston, Illinois

John Morreall

I

SPECIAL LECTURES

1982 PRESIDENTIAL ADDRESS

INTRODUCING CHARLES F. HOCKETT'S PRESIDENTIAL ADDRESS

M. A. K. Halliday
University of Sydney

Professor Hockett has been a member of this Association since its beginning in 1974. This period, long though it is in our history, represents perhaps one fifth of his scholarly life to date, a life that has been extraordinarily productive and rich in attainment.

In introducing Professor Hockett tonight, I feel very conscious of being in the presence of many others who, both from their distinction in the field of linguistics and from their much longer personal acquaintance with our President, could claim this privilege with much more justice than I can. But I welcome the opportunity to pay my own tribute to his very considerable achievements.

Geoffrey Sampson, in his book Schools of Linguistics: Competition and Evolution, speaks of Hockett as the one to whom he owes the greatest debt. "Over and over again," he writes in the Introduction, "I have discovered the source of some idea which I had fondly imagined to be original, on re-reading his State of the Art or another of his publications."

It is significant that Sampson referred to the State of the Art, because it is here perhaps more than anywhere else that Hockett demonstrates his remarkable ability to be at the same time both a practitioner in, and a critical observer of, his own chosen discipline. Many other linguists have been trained as anthropologists; but none has so successfully anthropologized linguistics, or so revealingly interpreted its particular academic sub-culture.

Not that State of the Art was a new departure for Hockett in its concern with the foundations of linguistics. Among his earlier articles, both 'Two Models of Grammatical Description' (1954) and 'Linguistic Elements and their Relations' (1961) were outstanding in the insights they gave into current principles and practice. His 1958 Course in Modern Linguistics abounds with thoughtful, and thought-engendering, observations on linguistics as a systematic pursuit. And in his presidential address to the Linguistic Society of America in 1965, multivalently entitled 'Sound Change', he charted the high tides in the ebb and flow of linguistic theory during the previous hundred years.

But State of the Art was much more than a commentary on the times. Its immediate context was a critique of current American linguistic theory, in the course of which, observing his new workmates as they threshed their own way through the history of the subject, triumphantly proclaiming each new rediscovery as they translated it into the language of their own reality construction, he gently remarked that "it is wasteful for the ontogeny of a

'school' to recapitulate the phylogeny of a discipline". The underlying theme of the book, however, 'is not linguistics but language, which Hockett has always recognized as the real domain - for linguistics did not evolve for the purpose of illuminating itself, despite the impression one may gain from much of one's reading in the subject. And here Hockett establishes one major and fundamental point: that although language is "characterized by various stabilities", it is not, in the mathematical sense of the term, a well-defined system. We can represent it as such only at the cost of reducing it to something other than it is: "the things left out of account ... are just the most important properties of human language". This point is elaborated in the preface to Language, Mathematics and Linguistics in a very characteristic passage (p. 10):

"For, if a language is in fact not a well-defined system, then what is the point of this, or any other, elaboration of algebraic grammar? It is easy to say that, even if the basic assumption of algebraic grammar is false, it may nevertheless afford us a useful approximation. Since this is easy to say, I hereby say it, thus offering some justification for the republication of this essay. But there is a much more important point. I now believe that any approximation we can achieve on the assumption that a language is well-defined is obtained by leaving out of account just those properties of real language that are most important. For, at bottom, the productivity and power of language - our casual ability to say new things - would seem to stem exactly from the fact that languages are not well-defined, but merely characterized by certain degrees and kinds of stability. This view allows us to understand how language works, how language changes, and how humans, using language, have created the well-defined systems of mathematics - for well-definition is born of stability through certain tricks of which only a speaking animal seems to be capable."

Every linguist is indebted to Hockett for his readiness, and his ability, to re-examine the basis of his own practices and assumptions in the light of new belief systems. He has never been afraid to admit he was wrong, and has been ready to reject both what he called the "post-Bloomfieldian atomic-morpheme two-stratum marble slab grammar", with its rigidity, its commitment to I.A. (item and arrangement) and its separation of grammar from semantics, and the "futile exercise in abstraction" of the formalist grammars that succeeded it. But whilst many of us were content to go about our own business, as far as we were allowed to, sensing intuitively that the latter exercise was in fact futile, Hockett examined it, tried it out, and gave detailed principled reasons for rejecting it. And he did this while laboring under conditions which must have made it extremely difficult for him to work at all.

The minor tribulations of a few privileged intellectuals are of no significance in the long context of history; and when major human catastrophes can so readily be laundered from memory it would

be gross to expect that our own small discomforts should have a place in the permanent record. But if only for this reason I would like to recall (to some, and perhaps intimate to others) the oppressiveness that was characteristic of those times, in which many aspiring linguists simply turned to other things; while others had to spend their most productive years defending their right to exist, instead of pressing on with the descriptive work that so urgently needed pursuing. Those who were active then recognize Charles Hockett as one who neither turned back, nor skirted round-about, nor closed his eyes till the cloud had passed, but went through with eyes open and came out the other end. On the way, he continued to behave as a linguist, offering ideas to the new masters of which one rated a footnote in Chomsky's Aspects of the Theory of Syntax (p. 223, n.3):

"In Hockett (1961) the proposal is made that the passive transformation be conditional on a marker in its underlying form; but no supporting argument is given for what, in the context of that paper, is no more than a notational innovation".

In other words - Hockett has made a good suggestion; but since he made it for the wrong reason, it doesn't count.

When he collected some of his essays, in a volume produced in 1977, Hockett called the collection The View from Language. The contents are broad in scope, ranging from linguistic topics such as (among many others) jokes and slips of the tongue, to the epistemology of history in which an account of the comparative method in linguistics leads him outwards to a consideration of physical systems and their past and future states. Here Hockett looks out from language on to the rest of the universe; and it is this breadth of vision that has given him such a balanced perspective on language itself, seen in his informed and thoughtful attitude to universals, to the nature of grammar, to language development in children, to langue and parole, to rules and regularities - an attitude that is always refreshingly free from the intrusion of fads and fantasies. It is not surprising that, faced with any one of a whole typology of problems, one can find oneself asking "I wonder what Hockett has to say about that?" So I hope he will go on having things to say, about whatever he chooses to be interested in, for a very long time to come.

Ladies and gentlemen, it is my great pleasure to introduce Charles Hockett, President of LAOS, to address us on the subject of "The Changing Intellectual Context of Linguistic Theory".

THE CHANGING INTELLECTUAL CONTEXT OF LINGUISTIC THEORY

C. F. Hockett
Cornell University

0. Introductory. In studying the history of our discipline it has been our tendency to focus on what each generation has inherited from its predecessors and on how each has modified that heritage before passing it on. But from Rask and Grimm to the present, linguists have never worked in isolation. All have been influenced directly or indirectly by the general scholarly climate of their times, and by current discoveries in other disciplines. I believe there have been various developments in our own field that can be fully understood only in the light of this constantly changing larger intellectual context.(1)

As a preliminary example, consider Grimm's career. Jacob Grimm (1785-1863) lived his life and conducted his research in the atmosphere of romanticism. Earlier European students of language had inclined to deal only with the "best" speech and to treat it prescriptively, grounding their assertions not in observation but in "logic." Grimm, on the contrary, insisted on empirical evidence, and felt the usages of lowly peasants to be just as important as those of royalty--perhaps in some ways even more important. The romantic spirit may explain the change. It focused attention on antiquities; also, in synergy with the democratic urge that was developing apace in this same period, it stressed the dignity of ordinary people. Jacob's collaborative work with his brother Wilhelm (1786-1859) on Germanic folk literature was totally in keeping with this spirit, and when he turned from folklore to language he took the romantic attitude with him. Perhaps we no longer think of ourselves as romantic, but many features of Grimm's stance are obviously still with us.

A thorough survey of the topic of this paper would of course require years of research. This brief essay pretends to no such resplendent status. I have striven to avoid serious anachronisms, but the exposition is intended only as suggestive, not definitive.

1. A Setting; Laplace. Much of what I have to say is about the nineteenth century. Let me therefore begin somewhat as the century did, with a germinal passage from the Philosophical Treatise on Probability, first published in 1814, of Pierre-Simon Laplace (1749-1827). Near the outset of his exposition, he says(2)

... we must view the present state of the universe as the result of its preceding state, and as the cause of that which will follow. An intelligence which, at a given instant, could know all the forces operating in nature and which moreover was powerful enough to submit those facts to [mathematical] analysis would subsume in a single formula the motions both of the largest bodies in the universe and of the lightest

1 Some of the substance of this paper was delivered as the author's presidential address to the annual meeting of the Linguistic Association of Canada and the United States in Evanston, Illinois on 5 August 1982. For this published version I have had the benefit of wise counsel from a number of colleagues, which I hereby acknowledge with thanks; all errors, of course, are mine.

2 The translation is mine (and rather free), from a 1921 reprint by Gauthier-Villars, Paris, of the 5th edition, 1825, the last edition to appear before Laplace's death.

particles; nothing would be uncertain for it, and the future, like the past, would be present to its eyes. The human mind offers, in the perfection it has been able to bring to astronomy, a feeble hint of that intelligence. All efforts in the search for truth tend always toward such an intelligence ..., remaining yet always infinitely removed from it. This tendency, characteristic of the human species, is what renders us superior to animals, and it is progress in this category that characterizes nations and centuries and forms their true glory.

Laplace here proclaims in ringing tones two themes that were to dominate the intellectual climate of European civilization for many decades.

One is the idea of progress: not the common medieval conception of the advancement of the individual soul, via obedience to authority in an essentially static world, toward possible admission to a Heavenly City, but, rather, the secular perfectibility of humanity, our collective march in this life in the direction of increasing goodness, truth, and beauty. Although this motif is touched on by Laplace, he did not originate it. It was the product of the two hundred years of the Enlightenment, culminating in the formulations of the (chiefly) French philosophes whose times just preceded Laplace's; and although many of the other notions of that period were quickly eroded, this one endured.⁽³⁾ It is difficult to find a nineteenth-century thinker who did not in some way respond to this doctrine, and of those who did, only a few were led to reject it. For the most part, individual investigators contributed their mites with good heart and with no misgivings as to the worth of their endeavors: they were consciously playing their diverse parts in that collective march. Even as late as 1898, when Alfred Russel Wallace (1823-1913), one of the two independent discoverers of natural selection as the mechanism of organic evolution, wrote a retrospective book about his century, he devoted half again as much space to its horrors as to its achievements--yet chose an upbeat title and ended on a note of unshakeable optimism.⁽⁴⁾

More than that, it is hard to see how anyone could have been led to the formulation of some of the crucial scientific theories of the nineteenth century, especially the theory of organic evolution, except in the context of the doctrine of progress. In a world conceived to be static one is not concerned with origins, for everything began at once, with the creation, just as all will collapse at the same time at the millennium.

The other motif highlighted in our quoted passage may also not have originated with Laplace, but his expression of it is so eloquent that even today we typically refer to it as "Laplacean determinism," and often assign to a hypothetical "Laplace's demon" the all-embracing intelligence he describes. Much of Laplace's share of the glory of which he speaks was that, as an astronomer and mathematician, he had struggled successfully with the difficult problem of taking into account more factors than had his predecessors in computing the motions of heavenly bodies, thereby being able to predict their observed positions more accurately. His determinism was the obvious extrapolation from that: the more we know, the more accurate are our predictions; in the limit, then--even though we can never attain it, only approach it asymptoti-

3 See Carl Lotus Becker (1873-1945), The Heavenly City of the Eighteenth Century Philosophers, 1932.

4 Wallace's title was The Wonderful Century.

This particular spirit of the century (there were others, to be sure) was perhaps best summed up at its very end, by the eighth symphony (the "Symphony of a Thousand") of Gustav Mahler (1860-1911), premiered in Munich on 12 September 1910: soloists, vast choruses, and an enormous orchestra, setting forth first a medieval Latin hymn and then the final scene of Part Two of the Faust of Johann Wolfgang von Goethe (1749-1842), dissolving at the end into a fortissimo burst of glory with the words Das Ewig-weibliche zieht uns hinan.

Then came the First World War.

cally--complete information would mean complete predictability; thus there is no objective uncertainty in the universe, and our need for probabilistic reasoning arises only from our ignorance.

2. The Great Dichotomy. In 1819, only a few years after the appearance of Laplace's treatise--and the year of publication of the first edition of Grimm's Deutsche Grammatik--Josiah Dwight Whitney Jr was born in Northampton, Massachusetts. William Dwight Whitney, better known to us linguists, arrived eight years later (three weeks before the death of Laplace, six before that of Ludwig van Beethoven).⁽⁵⁾ As they grew up, these two brothers decided not to enter any of the standard professions of the New England gentry, such as law, medicine, or the clergy, and not to follow in their father's footsteps as bankers, but to devote their lives to scholarship. I propose to use the Whitney boys, especially William, as protagonists for part of this story, thus somewhat personalizing the account. The reason goes beyond mere empathy. In the nineteenth century, New England was a favored provenience for a scholar, combining as it did the best of two sharply different worlds. With modest financial resources, the great European centers of learning were accessible. In the other direction, also easily reached, was the American sector of the spreading frontier of European civilization: in the 1820s it had barely crossed the Mississippi; in the 1890s, during which decade the lives of the Whitney brothers ended (William's in 1894, Josiah's in 1896), it had still not quite vanished. Like most American scholars of that time, Josiah and William studied in Germany.⁽⁶⁾ Unlike most, they also both went West, Josiah for a major part of his professional career, William only twice but each time to a region where pioneer conditions prevailed. To be sure, many who stayed home nevertheless felt indirectly the impact of the frontier experience, since the East Coast, however well protected against the rudeness and dangers of the westward-moving cutting edge, was close enough to feel the spirit of independence and self-reliance engendered by it.⁽⁷⁾ But for the Whitneys this effect was greatly heightened, while any tendency toward spurious romanticizing was quickly quashed.

The decision of the brothers to devote their lives to scholarship imposed on each a dichotomous choice, because in the climate of opinion of the time all fields of knowledge belonged to one or the other of two great branches of learning. Laplace had called these la philosophie naturelle and les sciences morales, but the German terms Naturwissenschaft and Geisteswissen-

5 There were other siblings (fourteen in all, though some did not survive infancy) to carry on at home in Northampton. Information on the Whitneys is drawn from the following sources: Edwin Tenney Brewster (1866-1960), ed., Life and Letters of Josiah Dwight Whitney (Boston, 1909); Charles Rockwell Lanman (1850-1941), ed., The Whitney Memorial Meeting (Boston, 1897); Laura Beaumont Hadley Moseley, ed., Diary (1843-1852) of [her grandfather] James Hadley (1821-1872) (New Haven, 1951); Thomas Day Seymour (1848-1907) "William Dwight Whitney," American Journal of Philology 15.271-298 (1894).

6 Josiah studied in the 1840s with the geologist Elie de Beaumont (1798-1874) in Paris, then with the chemist Justus Liebig (1803-1873) in Giessen; in Berlin he worked in the Rammelsberg laboratory. William's teachers, in the 1850s, were Franz Bopp (1791-1867), Albrecht Weber (1815-1901), Karl Wilhelm Ludwig Heyse (1797-1885), and C. Richard Lepsius (1810-1884) in Berlin; in Tübingen, Rudolph Roth (1821-1885), of whom he became more collaborator than student.

7 See Frederick Jackson Turner (1861-1932), "The Significance of the Frontier in American History," delivered orally in 1893, with many printings thereafter (e.g., in his The Frontier in American History, 1920). See also C. F. Hockett, Man's Place in Nature (New York, 1973), pp. 298-9. It is not beside the point to recall here that A. R. Wallace and Charles Robert Darwin (1809-1882), both English country gentlemen, also reached the edges of expanding European civilization (and beyond): Wallace for two years in Amazonia and ten in Malaysia, Darwin for five years on the Beagle.

schaft will better serve our purposes. Both Whitney boys had a true catholicity of intellectual taste and ability, and they ended up splitting the learned world between them. Josiah, though interested in language and Sanskrit before William was (in Berlin he attended lectures by Grimm and Bopp and brought back to Northampton the Bopp Sanskrit treatise that got William started), chose *Naturwissenschaft*, specializing eventually in economic geology, while William, though well trained as a naturalist, by focusing on oriental and linguistic studies committed himself to *Geisteswissenschaft*.

The word naturalist, which I have just used, was at the time the commonest English term for a worker in *Naturwissenschaft*. The German word for the category was sometimes rendered in full as natural science. From that, after the Englishman William Whewell (1794-1866) had in 1840 invented the agent noun scientist, one could have formed the rather awkward label natural scientist. More often, however, the first word was omitted in both these phrases, so that the bare terms science and scientist soon came to be understood as they still most commonly are today: that is, in the absence of explicit signals to the contrary, they were taken as referring only to *Naturwissenschaft*, rather than to the totality of *Wissenschaft*.

A letter written by Josiah to his sister Elizabeth in January of 1840 contains the term in its older, broader sense, sounding a bit strange to us today despite the modifier:⁸

You almost petrify me with horror at the bare mention of the books you are studying. I do hope you won't take to writing on Metaphysics, for of all the branches of human Science for a young lady to dip into, that seems to me the last.

But in the 1850s and 1860s, when a group of American men of learning sought to persuade the government to establish a national academy of sciences on the model of the British and French academies, it was clearly the narrowed sense of the word that was intended. As evidence, note a letter written by Harvard's Louis Agassiz (1807-1873) in 1858, listing the fields he thought should be represented by charter members in the proposed academy.⁹ His list begins with mathematics and astronomy (just Laplace's fields!); then physics and chemistry; then geology and mineralogy; then botany, zoology, and paleontology; and finally anatomy and physiology. He was hesitant as to whether "applications of science" should also be included, but proposed that, if they were, then agriculture, physical geography, navigation, medicine, and engineering warranted careful consideration.

Significantly absent from this list are philology, psychology, history, the "sociology" recently (1830) invented by Auguste Comte (1798-1857), and other more or less well known disciplines that obviously belonged to *Geisteswissenschaft*. No one disdained these fields. They were not inferior; they were simply different. Since they involved human motives, actions, and institutions, it was felt that they could not be dealt with by the methods of what Agassiz was calling "science"--that is, natural science, *Naturwissenschaft*. At the same time, we notice that he was by no means secure about the exact location of the boundary between the two overarching categories. His uncertainty about "applications" indicates this. One could argue for their inclusion on the grounds that they are, after all, applications of *naturwissenschaftlichen* findings; but it could be countered that they are disqualified because application necessarily involves human motives and decisions.

3. The Decay of Vitalism. Fifty or sixty years earlier, one of the fields Agassiz included with no comment, namely physiology, would probably have been challenged or excluded. Before 1800 there hardly was any physiology in the sense in which Agassiz meant and we mean the term,

⁸ *Life and Letters* (see *supra*, fn. 5), p. 36.

⁹ Rexmond C. Cochrane, *The National Academy of Sciences: The First Hundred Years, 1863-1963* (Washington DC, 1978), p. 46.

and the gulf between the inanimate and animate worlds seemed as impassible as, for Agassiz and his contemporaries, was that between the nonhuman and the human. Botanists and zoologists before 1800 observed in the field, collected, noted geographical distributions, and classified. They learned anatomy by dissecting plants and cadavers. But they had few means of investigating the intimate internal processes of living things. The lack of physiological information encouraged the generally held view that the comportment of an organism is under the control--potentially whimsical--of a special vital force that is absent in inert matter and that defies exact analysis of the kind so fruitful in physics and just then beginning to be so fruitful in chemistry. This belief, in turn, tended to discourage the search for techniques of physiological study. It is from this period, with its emphasis on field observation, collection, and classification, that we inherit the term natural history, about which more in a moment.¹⁰

By mid-century, when Agassiz wrote his letter, several things had happened.

First, in the face of practical difficulties and despite his belief in vitalism, Marie François Xavier Bichat (1771-1802) had gotten physiological study going, especially histology (but only with the naked eye: microscopes were at the time so poor that Bichat would not use them). His work was carried on by François Magendie (1783-1855) and Claude Bernard (1818-1878); it was the latter who gave to the term physiology the sense in which we understand it today.

Along a different line, the use of nitrous oxide, ethyl ether, and chloroform as anesthetics got started in the 1840s at the hands of two American dental surgeons (Horace Wells, 1815-1848, and William T. G. Morton, 1819-1868), an American surgeon (Crawford W. Long, 1815-1878), and a Scots obstetrician (James Y. Simpson, 1811-1870). The introduction of anesthetics into the physiological laboratory enormously enhanced the experimental possibilities.

And in a totally different context, the concept of energy was clarified and the equivalences among its different manifestations were established. Most recalcitrant in this enterprise was heat. But the work of James Watt (1736-1819), culminating in his perfection of the condensing steam engine in 1769, led, through the discovery of the Carnot cycle in the 1820s (Sadi Carnot, 1796-1832), to the mid-century formulation by such investigators as Rudolph Clausius (1822-1888) and William Thomson (Baron Kelvin, 1824-1907) of the two great laws of thermodynamics, the first of which is the law of the conservation of energy.¹¹

By the half-century these lines had converged. Experimental methods in physiology had improved to the point that it was demonstrated that the laws of thermodynamics hold in organisms just as they do in nonliving systems. The crucial experiments were those of Theodor Schwann (1810-1883), J. Robert Mayer (1814-1878), and Hermann Helmholtz (1821-1894).

That was the second strike against the vitalist view. The first was the synthesis of urea by Friedrich Wöhler (1800-1882), in 1828--incidentally the year of Franz Schubert's death and of Jules Verne's birth. The vitalist view had held that "organic" compounds, meaning those found characteristically and exclusively in organisms, could not be built up out of raw-materials except under the direction of the posited vital force. Wöhler's synthesis was evidence to the contrary. His procedures were subject to valid challenge, but they pointed the way, and in the ensuing decades literally thousands of other organic substances were put together in the laboratory under conditions which were unquestionably "inorganic."

The voices of vitalism were not silenced by these developments--they are still to be heard today--but they were considerably muted. It became not just respectable but wise, whatever one's beliefs, to conduct biological experiment on the working assumption that there is no vital force, since if in fact there is its effects will sooner or later become evident. Actual-

10 For this section I have drawn on L. Pearce Williams and Henry John Steffens, The History of Science in Western Civilization, volume 3, Modern Science, 1700-1900 (Washington DC, 1978), supplemented by viva voce consultation with Professor Williams.

11 See Donald Stephen Lowell Cardwell, From Watt to Clausius: The Rise of Thermodynamics in the Early Industrial Age (1971).

ly, this was Bichat's policy (though differently expressed⁽¹²⁾) at the end of the eighteenth century, and at the beginning of the twentieth it was more reemphasized than clarified by the wise words of the founder of biophysics, D'Arcy Wentworth Thompson (1860-1948):⁽¹³⁾

We may readily admit ... that besides phenomena which are obviously physical in their nature, there are actions visible as well as invisible taking place within living cells which our knowledge does not permit us to ascribe with certainty to any known physical force: and it may or may not be that these phenomena will yield in time to the methods of physical investigation. Whether or no, it is plain that we have no clear rule or guide as to what is "vital" and what is not; the whole assemblage of so-called vital phenomena, or properties of the organism, cannot be clearly classified into those that are physical in origin and those that are sui generis and peculiar to living things. All we can do meanwhile is to analyze, bit by bit, those parts of the whole to which the ordinary laws of the physical forces more or less obviously and clearly and indubitably apply.

4. More on the Dichotomy. But if--when Agassiz wrote his letter--the gulf between living and nonliving thus seemed narrower, that between human and nonhuman yawned as unbridgeable as ever. It did not matter that human physiology is much like other physiology. Obviously human beings have bodies, which behave like matter because that is what they are. (Therefore there is such a thing as "human nature," whose study belongs to Naturwissenschaft.) But we also have minds, and mind is a different sort of substance, obedient perhaps to different laws. There are two categories of science because there are two fundamentally different kinds of things to be scientific about. We see that a primary source of the bifurcation of learning was a philosophical dualism whose origins lie so far back in the history of our species that we have no record of them; we can only speculate.

This basic distinction seems to have been accompanied by some others.

Thus, in the view of many the contrast set off the essentially repetitive and noncreative, on the one hand, from the cumulative or nonrepetitive on the other. In the realm of nature one can make timeless assertions: the valence of oxygen is two, always has been, and always will be, whether the oxygen be in someone's bloodstream or in an interstellar cloud. But in Geisteswissenschaft there can be no such generalizations. The way things are in the human world is constantly changed by the willful actions of people. Therefore only a particularistic approach is possible--whereupon the Geisteswissenschaften were also called the historical sciences, or just history. The nature-history contrast was exactly the same, though the emphasis was reversed, when in 1851 (according to Jespersen⁽¹⁴⁾) Grimm asserted that "Nature and natural instinct have no history, but mankind has." All this, of course, makes the phrase natural history, which we mentioned a moment ago, sound like a contradiction in terms. Maybe it was. But, at least, the phrase did connote a highlighting of the unique and ungeneralizable aspects of the environment.

Next, recall Laplacean determinism. Throughout the nineteenth century every experimental scientist acted on the assumption that Laplacean determinism is true. The assumption tells you how to try to control the conditions of experiments and how to interpret the outcomes. At the same time, people were convinced that we ourselves have free will. We can and do make choices. That is why there is such a thing as ethics, which in turn is why the historical sciences were also often called the moral sciences (for example--as we noted--by Laplace). Now, if mind is

12 Op. cit. (supra, fn. 10), pp. 311-2.

13 On Growth and Form (Cambridge, 1917), p. 14.

14 Jens Otto Harry Jespersen (1860-1943), Language: Its Nature, Development, and Origin (London, 1922), p. 61.

not a substance separate from matter but, instead, just a very complex emergent manifestation of matter and energy in a determinate universe, then free will is an illusion. Contrapositively, if free will is not an illusion, then mind is exempt from Laplace's formulation, and then also mind is a substance distinct from matter.

That dilemma, however disguised by diverse terminologies, was surely at the heart of the dreadful intellectual agony that followed the announcement, in 1858 and 1859, of the Darwin-Wallace theory of organic evolution, an agony that the essential optimism of the doctrine of progress could do little to alleviate. Even the two independent discoverers of variation and speciation by natural selection went separate ways on this issue. Darwin was consistent: he included our own species in his generalization. Wallace, apparently greatly to Darwin's disappointment, excepted man as surely having involved special divine intervention.<15>

5. The Place of Linguistics. So much for the intellectual backdrop against which the field of linguistics emerged. I doubt if what I have said is completely clear. In one sense it shouldn't be, since the situation in the nineteenth century was itself considerably confused. Also, the outline I have given is far from complete. But we will leave other threads in the tapestry to be teased into focus as the need arises.

That linguistics, Sprachwissenschaft, was not just to be identified with philology was agreed to by everyone. But was the study of language a natural science or a moral one?

August Schleicher (1821-1868), whose key contributions to the early development of Indo-European studies make him for us one of the immortals, assumed at first that it was a moral-historical discipline, but then, perceiving in language a kind of lawfulness characteristic of natural phenomena, changed his mind. In 1863, having just read Darwin's Origin of Species, he argued that linguistics is a natural science. We shall view his reasoning first through the eyes of our chosen hero, William Dwight Whitney, who in 1871, after Schleicher's regrettable early death, published a critique.<16> He quotes Schleicher as follows:

Languages are natural organisms, which, without being determinable by the will of man, arose, grew, and developed themselves, in accordance with fixed laws, and then grow old and die out; to them ... belongs that succession of phenomena which is wont to be termed "life."

Taken at face value, via Whitney's English translation, this strikes us as absurd, a metaphor gone wild. Whitney thought so too. He points out that from the fact that languages are in some interesting ways like organisms it does not follow that they are organisms. Yet there really is one singularly striking parallel between language and plants and animals. Schleicher and Whitney both knew that in course of time a single language (e.g., Proto Indo-European) can

15 I think the reference to Darwin's disappointment is correct, but cannot at the moment document it.

16 "Schleicher and the Physical Theory of Language," in American Philological Society Transactions for 1871; reprinted in Whitney's Oriental and Linguistic Studies (New York, 1873), pp. 298-331; our first quote is from p. 300 and the second from p. 301. Schleicher's essays under attack were Die Darwinsche Theorie und die Sprachwissenschaft: Offenes Sendschreiben an Herrn Dr. Ernst Haeckel (Weimar, 1863), and Über die Bedeutung der Sprache für die Naturgeschichte des Menschen (Weimar, 1865). For recent reviews of Schleicher's work (fuller and probably fairer than my brief remarks), see E. F. Konrad Koerner, "Schleichers Einfluss auf Haeckel: Schlaglichter auf die wechselseitige Abhängigkeit zwischen linguistischen und biologischen Theorien im 19. Jahrhundert," Nova acta Leopoldina n.s. 54.731-45 (1981) and Victor H. Yngve, "The Struggle for a Theory of Native Speaker," in Florian Coulmas, ed., A Festschrift for Native Speaker, Janua Linguarum Series Maior no. 97 (1981), pp. 31-49.

proliferate and differentiate to become many languages. If the evolutionists were right, exactly the same can happen in course of time to a single animal or plant species. Apparently nothing else in the universe shows this behavior. Moreover, let us remember how very recent, at the time, was the full recognition of biology as deserving a place alongside physics and chemistry, and the continuing widespread belief in vitalism. Perhaps it was not so foolish, after all, to propose that plants, animals, and languages, all as then conceived, are sufficiently alike to merit assignment to a single larger category.

Beyond that, the crucial criterion--and here Whitney and Schleicher seem to have been in agreement--is the role of the human will. On this, Whitney says

If the voluntary action of men has anything to do with making and changing language, then language is so far not a natural organism, but a human product. And if that action is the only force that makes and changes language, then language is not a natural organism at all ...

but an institution. And many long and beautiful passages in Whitney's general works are devoted to the argument that voluntary action is indeed "the only force that makes and changes language."¹⁷

A closer look shows that the focal disagreement between Schleicher and Whitney was not quite what the latter's critique suggests.

They probably located the boundary between natural and moral science somewhat differently. True, both had been exposed to and stimulated by natural science (especially botany) in their youth, but Whitney's conception of it derived from considerable practical experience and from constant interchange with his brother, whereas Schleicher's was early overlaid by a strong wave of Hegelian influence. So if they had seen completely eye to eye about language itself, they might still have disagreed about the proper assignment of the discipline devoted to its study.

Georg Wilhelm Friedrich Hegel (born--like Beethoven--in 1770, died 1831) can justifiably be characterized as the philosophical spokesman for a way of thinking about time and change that turned up in geology as catastrophism, popular in the eighteenth century and still defended in the nineteenth by Georges Cuvier (1769-1832) in opposition to the uniformitarianism of James Hutton (1726-1797) and Charles Lyell (born, like Schubert, the year of Hutton's death; died 1875)--and of Darwin. Hutton and Lyell proposed that the forces responsible for past changes in the structure of the earth were just those we can still see at work today. Catastrophism proposed stages and steps: in a given stage, certain forces dominate, but in course of time they lead to the building up of internal stresses, which then bring about a relatively sudden restructuring (a catastrophe), initiating a new stage with new rules.¹⁸ Early views of language history reflect a similar conception. Thus Schleicher once held that each language passes in time through three stages: first isolating, then agglutinative, and finally inflectional--a sequence clearly regarded as progressive. To be sure, no one in historic times had observed a transition from one of these stages to the next. But the available evidence seemed to show that such transitions had taken place earlier, before the beginnings of record-keeping. One can also discern traces of an older model for part of this scheme, though it appears in clearer form in the work of such predecessors of Schleicher as Grimm and Wilhelm von

¹⁷ See especially the marvelous second lecture in Whitney's Language and the Study of Language (New York, 1867; 5th ed., 1884).

¹⁸ Since one doctrine has to do with how evidence should be interpreted, the other with how things happened, the two were not in direct conflict. But the premisses of each school led to inferences contrary to the teachings of the other. It is interesting that the bits of geology put into the mouth of Conseil in Jules Verne's Vingt mille lieues sous les mers (Paris, 1869) is still catastrophism.

Humboldt (1767-1835). This model is the medieval conception of the plot of universal history. As we remarked in section 1, the stage on which the human drama is enacted (the natural world) is essentially static. But the drama itself has a contour, something like a symmetric segment of a concave-downward parabola: everything began in a small way, rose (with some catastrophes, such as the Flood) to a climax in Christ's coming, and since then has been going downhill toward a dénouement in the form of Judgment Day. The ascending part of this curve is matched, in the Humboldt-Grimm-early-Schleicher portrayal, by the rise of languages from their putative isolating beginnings through agglutination to the classical inflectional perfection attested by Latin and Greek; since then, obviously, inflections have been wearing away. Even with Grimm some doubts appear as to whether the loss of inflections is really a change for the worse, though the clear triumph of the doctrine of progress over the medieval rainbow-curve conception came only much later, with Jespersen, whose *Progress in Language* appeared the year of William Dwight Whitney's death.

Of all this, we are sure of Whitney only that he shared with Schleicher, and with Bopp, the view that the "original" Indo-European language had been isolating. Indeed, Whitney argues at great length that the available evidence, properly interpreted by uniformitarian principles, allows of no other conclusion.¹⁹ (Remember that our own most prevalent views on such matters are much too recent to have played any part in the debate of which we are here speaking.)

In this context, the real divergence between the views of Whitney and of Schleicher was not whether individuals, within only the sorts of constraints we encounter in all our affairs, can choose what they are going to say and how to say it. Instead, it had to do with the long-term collective consequences of such choices.

Schleicher's reasoning, never fully elaborated, was Hegelian in the same way as was that of Karl Marx (1818-1883) at about the same time. Marx did not deny, for example, that an individual entrepreneur can try to be a generous fellow if he so chooses. He argued only that the capitalist system--the interlocking relationships among people within which their decisions lead to one or another result--involves internal stresses forcing it as a whole to develop in a certain way, resulting ultimately in a relatively sudden system-changing outcome (a revolution: the Hegelian leap from quantity to quality, the catastrophe of catastrophism). Schleicher's proposal for language, if worked out more fully, might have been something like that. Whitney might then have agreed, but in any case it seems likely that there would have been a major difference in emphasis. Whitney, true to the spirit of the American frontier, would have been much more concerned with the free-will nature of the individual decisions that lead(?) to the collective consequence; Schleicher probably would have been more intrigued by what he thought was the inevitability of the consequences.

Before passing on we must note that, despite the similarity of labels, neither "economic determinism" in the Marxist sense nor "linguistic determinism" in the sense a Schleicherian might give that phrase really has anything to do with determinism in Laplace's sense. With Laplace's codification of probability there is a very close connection indeed: Marxist theory--whether correct or not--affords a useful model of a state of affairs in which a large number of separately unpredictable small events (the decisions and actions of individuals) lead to a predictable collective result. This way of thinking must have been contagious, since in just those same years investigators like Ludwig Boltzmann (1844-1906) and Willard Gibbs (1838-1903) were using it in their refinement of phenomenological thermodynamics into statistical mechanics. But with Laplacean determinism there is no connection, since for the Marxian mechanism it matters not in the slightest whether the free will with which individuals makes their decisions

¹⁹ See, for example, the seventh lecture in Whitney, *op. cit. supra* n. 7. The notion died slowly. I find it challenged by implication, though not head on, in section 277 of Hermann Paul (1846-1921), *Prinzipien der Sprachgeschichte* (1880; my reference is to the 2d edition, 1886).

is a reality or an illusion.<20>

6. Scientific Methods. I hope I have not given the impression that there was anything wishy-washy about Whitney's espousal of the classification of linguistics as a moral science. At the same time, his feel for the proper conduct of all science derived not just from the armchair but also from his own experience as a naturalist and from constant contact with natural scientists (he was not his brother's brother for nothing). As a consequence, he had high regard for empirical methods--the only ones that work on the frontier, where preconceived fixed ideas can be fatal--and was seriously doubtful, though apparently not totally unbelieving, about the chances of getting ultimately worthwhile results in any branch of learning from pure speculation.

Some of his contemporaries felt otherwise; for example, Heymann Steinthal (1823-1899). Steinthal's noble aim was to free our understanding of language from direct dependence on metaphysics and to anchor it instead in psychology, a subject which, though in some ways ancient, was just being radically reworked under the influence of Johann Friedrich Herbart (1776-1841).<21> However, Steinthal's psychology--like Herbart's--was itself replete with metaphysics. Also, the eighteenth-century conviction that reason, unencumbered by data, could discover certain kinds of truth was still very much alive. One could have predicted that Whitney would see the worst side of Steinthal. In 1872 he published a critique, sounding a note that is perhaps more familiar to us today, a century later, than it should be.<22> Says Whitney,<23>

... as we have hinted already, [Steinthal] is nothing if not metaphysical, and the metaphysical method requires that one get behind the facts he deals with, and evolve them by a necessity out of some predetermining principle. This is the opposite of the current scientific method, which is proud to acknowledge its dependence on facts, and prefers to proceed by cautious induction backward from the known and familiar to the obscure and unknown. Both methods ought to come to the same thing in the end [here we discern Whitney's honest doubt about his methodological preference], and will do so, provided they be conducted with sufficient reach and insight, and at the same time with sufficient moderation and caution; we are used, however, to seeing the metaphysical, when it comes to dealing with concrete facts and their relations, fail by labored obscurity or by forced and distorting treatment. The result alone can decide which is the better, as applied to language.

And in another place,<24>

... whether the advance of psychology is or is not to bring about a revolution in the science of language, is a question depending on the manner and degree in which language is a "mental production." It is very possible here to fall into the serious error of looking upon words and phrases as an immediate emanation of the mind, and so of settling the laws of mental action, and out of them evolving the events of lan-

20 A. d'Abro, The Rise of the New Physics: Its Mathematical and Physical Theories (New York, 1951; revision of his The Decline of Mechanism, 1939), chapter 22.

21 Steinthal's Einleitung in die Psychologie und Sprachwissenschaft (Berlin, 1871) revises the views expressed in earlier works.

22 So says Erwin A. Esper (1895-1972), Analogy and Association in Linguistics and Psychology (1973), p. 19, fn. 11.

23 "Steinthal and the Psychological Theory of Language," in The North American Review, vol. 114; reprinted in Oriental and Linguistic Studies, pp. 332-375; the passage is on pp. 334-5.

24 Op. cit., p. 343.

guage-history. The soul of man and its powers and operations are, after all, the mystery of mysteries to us; the phenomena of language are one of its external manifestations, and comparatively a simple matter; the light which these shall cast upon the soul must probably be greater than that which they shall receive from our comprehension of the soul. If the linguistic student, in his devotion to psychology, shall invert this relation, he is very likely to add one more to the already numerous instances in which metaphysics has shown its inaptitude for dealing with facts of observation and induction.

We are left with no doubt as to Whitney's belief as to where investigation should start and the direction in which it should move, or as to his faith in a real difference between the natural and the moral sciences, in the proper classification of linguistics among the latter, and in the wisdom of making haste slowly.

What is not clear is whether he kept to the traditional dualistic view of mind and matter through fundamental conviction or because, on this issue also, he felt it wiser to proceed with "moderation and caution."

This, note well, is not the problem of where linguistics belongs. That is already settled, and it belongs with the moral sciences. This is, rather, the very much more profound problem of the ultimate nature of all moral science. Is the fundamental dichotomy perhaps, in the last analysis, a false one? If vitalism is wrong, so that living forms arose from inert matter early in earth's history entirely through the operation of natural forces, and if human beings are animals, as the theory of evolution proposes--not "mere" animals, but animals just the same--then must not moral science be a branch of natural science, rather than a contrasting category?

People were thinking a lot about this issue in the decades after 1859, and if their ideas were often somewhat confused, from the ninth decade of the following century I can find no finger of scorn to point at them. Whitney's position is also not entirely clear. Certainly he was not favorably impressed by the²⁵

headlong materialists who pronounce man the slave and sport of nature, guided and controlled by the external forces amid which he exists, and who claim that his history may be explained and foretold by means of a knowledge of those forces; when as yet they have not found out even the A-B-C of the modes in which human nature is moulded by its surroundings.

Elsewhere he expands this a bit, emphasizing that language cannot in this connection be divorced from other aspects of human conduct:

There is a school of modern philosophers who are trying to materialize all science, to eliminate the distinction between the physical and the intellectual and moral, to declare for naught the free action of the human will, and to resolve the whole story of the fates of mankind into a series of purely physical effects, produced by assignable physical causes, and explainable in the past, or determinable for the future, by an intimate knowledge of those causes, by a recognition of the action of compulsory motives upon the passively obedient nature of man. With such, language will naturally pass, along with the rest, for a physical product, and its study for a physical science; and however we may dissent from their general classification, we cannot quarrel with its application in this particular instance.

²⁵ Language and the Study of Language, p. 153 in the 5th ed. (1884); the next passage is from p. 49 of that edition.

These passages certainly sound resolutely dualistic. But to be sure we should have to identify Whitney's unnamed targets. The wording is in part reminiscent of Laplace's, so he may have been speaking of serious and well informed scholars, such as Herbert Spencer (1820-1903) or Chauncey Wright (1830-1875), who tried very hard to accept the Darwinian dispensation without bowdlerization and think through its human implications.⁽²⁶⁾ But there were also numerous purveyors of a vulgar pseudophysicalism that insisted on a direct and drastic influence on human nature of climate, diet, the stars, or other such environmental factors--what we might characterize as snake-oil science. Whitney may have been thinking of them.

Whitney himself seems to have had no trouble accepting the Darwinist theory of human origins, but how he squared that with his belief in "the free action of the human will" is unrecorded.

7. Still More on the Dichotomy. That is as far along our path as we shall travel with Whitney as cicerone (though we shall hear his name again before we are done). He was entirely of the nineteenth century, and even some important developments of that century came too late for him to record any carefully pondered reaction. For his period, I do not see how one could hope for a saner, more hard-headed, more sensibly balanced view. If we differ from him today on many issues, it is because of evidence available to us but unknown to him.

Before we move on, I must overtly acknowledge something the reader has surely already noticed and that did not pass unrecognized by our predecessors, although I have soft-pedaled it in this exposition for the sake of dramatic effect: that the superficially straightforward dichotomy of learning into *Geisteswissenschaft* and *Naturwissenschaft* was not really simple at all, but, rather, an uneasy conflation of a half dozen different dichotomous distinctions which don't altogether entail one another.

At bottom, as we have already noted, lies the ontological dualism of mind versus matter, or, perhaps better, of the mental versus the physical.

In epistemology there is the opposition between rationalism and empiricism, with Whitney speaking out for empiricism in the mental sciences as well as in the physical. (Perhaps he would have exempted mathematics from his recommendation had he known more of that field.)

In etiology we have the random, willful, and unpredictable, collectively the indeterminate, versus the determinate.

In temporal perspective we have the synchronic versus the diachronic, badly needed labels which became available as a contrasted pair only when Ferdinand de Saussure (1857-1913) invented the second at about the turn of the century; even today they are rarely heard outside our own discipline.⁽²⁷⁾

Within the diachronic, for the contours of what happens as time passes, there is the contrast between the dynamic, evolving, or aperiodic versus the repetitive, periodic, or stationary.⁽²⁸⁾ The Hegelian dialectic in the philosophy of history is an aperiodic paradigm. The cliché "history repeats itself," if taken seriously, would be a stationary theory of change. The devisers of the comparative method in linguistics thought they were establishing it on an aperiodic foundation, for they were interested in origins; they would be disappointed to learn, as most of us now believe, that the method yields reliable results only on a stationary ba-

26 One of Wright's essays, "Evolution of Self-Consciousness" (1873), is conveniently reprinted in Perry Miller, ed., American Thought: Civil War to World War I (1954), pp. 27-45.

27 In Tullio de Mauro's critical edition (1967) of Saussure's Cours de linguistique générale, editorial note 170 says that "diachronic" appears for the first time in a notebook of Saussure's that dates later (but not much later) than 1894.

28 For formal use this term, borrowed from probability and statistics, seems better than "static," to which I resorted earlier in this essay. "Static" tends to imply that nothing happens; "stationary" means only that the patterns or probabilities for events remain constant.

sis.<29>

Different from all these is the contrast between two opposite directions in which research can be aimed: toward the precise characterization of specific events or states, or toward the formulation of more or less wide generalizations. In 1935 Alfred L. Kroeber (1876-1960) published a fine paper entitled "History and Science in Anthropology"<30>; by "history" he meant the first of these two research orientations, by "science" the second, and that way of using these two words has been very common. But back in 1891, in a monograph entitled *Geschichte und Naturwissenschaft*--virtually Kroeber's two terms in German dress and reverse order--Wilhelm Windelband (1848-1915) argued that these two words also have many other meanings, so that it would be advisable to have separate labels for the research directions.<31> He proposed *idiographic* and *nomothetic*. The American psychologist Gordon W. Allport (1897-1967) thought these terms very useful.<32> The British historian-philosopher Robin G. Collingwood (1889-1943) found them pompous.<33> But if one is trying to untangle the tangled web concealed by the simple pairing of *Geisteswissenschaft* and *Naturwissenschaft* I don't see how one manage without them. It is necessary to warn that whether an assertion is idiographic or nomothetic can depend on the context. In an ethnographic study of the Yurok Indians the statement that their houses have gabled roofs with triple pitch is nomothetic--a generalization. In an ethnological survey of aboriginal North American housing types the same statement is idiographic.

As an annoying but obligatory codicil to the foregoing, we must remark that the word *history* turns up also in some usages in which it is not in any way contrasted with *nature*. For example, it is sometimes implied that the past is history and the present is not (or not yet). In another colloquial sense, only events important enough to deserve a place in the history books count as history. It should not be too hard to avoid confusion from these angles.<34>

This more elaborate terminology, with its six (or more) linearly independent dimensions of contrast, can help us as we try to puzzle out why certain themes in the history of linguistics have developed as they have.

8. Laws. First: why were sound shifts called *laws*?

Like many scholars of his generation, Jacob Grimm had early legal training. Yet in 1822, when he published the second edition of the first volume of his *Deutsche Grammatik*, presenting the discoveries of Rasmus Kristian Rask (1787-1832) along with additional examples and his own reinterpretation, he did not use the term "law." In fact, there is some question whether these earliest formulations (by Rask, Grimm, and their predecessors) were intended as synchronic assertions of sound correspondences, as diachronic descriptions of changes between stages, or somehow as both. By mid-century the diachronic view had emerged clearly enough for people to speak of *Lautverschiebungen*; and by that time the concept of law had also been introduced, so that one tended to replace *Lautverschiebung* by *Lautverschiebungsgesetz* or, even more often, just by *Lautgesetz*.<35> In English, the phrase "Grimm's law," a misnomer twice over, familiar

29 C. F. Hockett, "In Search of Jove's Brow," *American Speech* 53.243-313 (1978), pp. 257-62.
30 *American Anthropologist* 37:4.539-69.

31 Reprinted in Windelband's *Präudien* (5th edition, Tübingen 1915), vol. 2, pp. 136-60.

32 See, e.g., his *Personality: A Psychological Interpretation* (New York, 1937), p. 22.

33 *The Idea of History* (Oxford, 1946), pp. 166-8.

34 Somehow I can't seem to finish this catalog! I have left out the very common use of "history" to subsume only that part of the past known through written records (versus "prehistory") and also the distinction between "history" as the study of the past [the original meaning of the term in the usage of Herodotus (5th century BC), now often also called "historiography"], and as the past that is studied. I can only pray that this radical polysemy will not obscure my message.

35 E.g., Theodor Benfey (1809-1881) in the chapter about Grimm in his *Geschichte der Sprach-*

today to millions of people who otherwise don't know a fricative from a fricassee, was the contribution of Max Müller (1823-1900).^{<36>} Although one can validly assert, as Leonard Bloomfield (1887-1949) did in 1933, that this use of the word is a dangerous metaphor, that does not explain how the usage arose.^{<37>}

The word "law," of course, has its own very complex history. We tend to forget that only a few centuries ago the phrase "natural law" referred not at all to generalizations (nomothetic statements) like Newton's laws of motion or the laws of thermodynamics, but to those rights and duties of human beings toward one another that were assumed to be inherent in the fabric of the universe--such as the king's right to rule, the subject's duty to obey. But by the nineteenth century the commonest context for the word (outside the legal profession) was probably that of natural science. A law was typically diachronic, determinate, and considered to be not subject to interference or alteration by the human will. Even so, some investigators, as though by a sort of cross-category analogy, were searching for "laws of history," meaning, I guess, something like recurrent and perhaps unavoidable regularities in the unfolding of human affairs.

By contrast, a sound shift would seem to us almost inevitably idiographic, like the invention of the alphabet or Cortez's conquest of Mexico. But it is possible to imagine lines of association like the following. The phenomenon discovered by Rask and by Grimm had to do with sounds. The sounds of speech are on the physical side of language even though under mental control. Phonetics thus has some claim to be recognized (like anatomy and physiology) as a natural science. Furthermore, the phenomenon showed a pervasive regularity in the sphere within which it was manifested--many words were reshaped in the same way. Also, it seemed not to have any connection with the human will: the changes in pronunciation seemed more like something happening to people than something done by people (though on this point Whitney might have demurred). Moreover, early in the game it was suspected--wrongly, as was soon recognized--that the same thing (a sort of "rotation of consonants") had happened at least twice, once between ancestral Indo-European and early Germanic, again between early Germanic and High German. Surely recurrence is suggestive of law. By the time of the Junggrammatiker it was obvious that a sound shift was, in a sense, a mass phenomenon, affecting many people at once; and by then it has also been shown by statistical mechanics (as we noted at the end of section 5) that the great principles of thermodynamics, always called "laws," also had to do with mass phenomena. Finally, apart from all else, we know that "law" was at the time a highly prestigious word. Given all that, the use of the term would seem to have been not at all far-fetched or surprising.

Once introduced, the term persisted, as usages often do, through mere inertia (we linguists, of all people, should understand that). In the same way, we still speak of malaria, although we know perfectly well that the disease has nothing to do with bad air and are not misled by the etymology. But in the case of Lautgesetz the connotative ties seem never to have been completely severed. I am convinced that they played a part in leading the Junggrammatiker to phrase their hypothesis as they did--August Leskien (1840-1916) does use the term Lautgesetz in his formulation of 1876,^{<38>} however mild and nonbelligerent his wording--and their opponents to reply as they did, in the imbroglione that has gone on now for well over a hundred years with still no resolution in sight.^{<39>}

wissenschaft und orientalischen Philologie in Deutschland (Munich, 1869), reprinted in Thomas A. Sebeok, ed., Portraits of Linguists (Bloomington IN, 1966), vol. 1, pp. 120-54.

36 So says Jespersen, op. cit. (supra, fn. 13), p. 43.

37 His Language, section 20.3.

38 Die Declination im Slavisch-Litauischen und Germanischen (Leipzig), p. xxviii.

39 The title "Resolving the Neogrammarian Controversy" that appeared in Language 57.267-308 (1981), whether accurate or not, was, in my opinion, an impropriety that the journal's editor should not have permitted.

9. Bloomfield's "Two Traditions." Next we ask: when Hermann Paul named his 1880 treatise Prinzipien der Sprachgeschichte, what did he imply by this choice of title rather than, say, Prinzipien der Sprachwissenschaft?

Let us recall how Bloomfield handled this issue. In the first chapter of his 1933 book Language he speaks of two largely distinct traditions of linguistic research in the nineteenth century, which he labels (p. 19) the "historical-comparative" and the "philosophical-descriptive." By the first he clearly means the study of language change: diachronic analysis, both of specific languages and in generalization therefrom, together with reconstruction, interpreted as affording a point d'appui for change. By the second label he means synchronic scrutiny, likewise both idiographic and nomothetic. Paul's work is assigned to the first of these two traditions. Thus the term Geschichte in Paul's title is being understood primarily in the sense of diachrony in contrast to synchrony.

There is no question about the reality of the synchronic-diachronic distinction in language study, nor would anyone deny that some investigators have been more taken by one perspective, some by the other. Yet I want to suggest that Bloomfield's interpretation may reflect, in part, a misunderstanding.

Our discussion in earlier sections has shown how multifaceted was the term history throughout the nineteenth century. Not much earlier than Paul, his choice of title could have been merely an acknowledgment that the study of language belongs to Geisteswissenschaft rather than to Naturwissenschaft. Furthermore, although in Paul's Prinzipien there is truly a great emphasis on language change, apparently I find a greater share of synchrony in it than Bloomfield did. Some of it, true enough, is cast in what Bloomfield thought of as psychological terms, but that does not keep it from being synchronic.⁴⁰ Also, it is understandable that Paul's occasional synchronic nomothetic assertions are faulty, since he had (or, at least, he made use of) very little reliable information on languages other than those of Europe and their Indo-European congeners.

The misunderstanding here is the notion, held by or ascribed to some scholars and rejected by others, that the only valid approach to language is the diachronic one.

I think that notion could arise as the "conclusion" from two "premises": first, that since linguistics is a Geisteswissenschaft it must be historical; second, that since linguistics is historical it must be diachronic. Of course that is "Tarzan thinking" (undistributed middle: you swing on a single term from one of its meanings to another, as Tarzan swung on a hanging vine from one tree to another). 'Diachronic' is one of the meanings of historical, but by no means the only one, and although moral-science research is often historical in one or another of these senses, it is not invariably so. What is more, by Paul's times historical study (in some senses of the term) was no longer inevitably Geisteswissenschaftlich, for it had become clear that not only human affairs grow and change but also life forms and even earth itself.

No respectable scholar would ever seriously have entertained our "syllogism." I am not proposing that it was entertained. I am proposing that it sneaked in behind the scenes, in the largely unconscious terminological frame of reference within which we do our in-awareness theorizing. We cannot constantly drag all of our vocabulary into the focus of attention for semantic dissection and purification--if we tried to, we should never get anything said.

I have not yet tracked down when and by whom these associative pseudo-inferences were made. It may have been Bloomfield. It may have been Berthold Delbrück (1842-1922), or his opponent Wilhelm Wundt (1832-1920), in the great debate of 1901.⁴¹ It may have been the early Jung-

40 Bloomfield, op. cit., section 1.8, last paragraph.

41 Wundt, Die Sprache (volume 1 of Völkerpsychologie) (Leipzig: Engelmann, 1900; third and final edition in two volumes, 1911-1912). Delbrück's critique: Grundfragen der Sprachforschung mit Rücksicht auf W. Wundts Sprachpsychologie erörtert (Strassburg, 1901). Wundt's reply: Sprachgeschichte und Sprachpsychologie mit Rücksicht auf B. Delbrücks "Grundfragen der

grammatiker, including Paul. It was not Whitney. Pending an investigation more detailed than I can at present undertake, my suspicion is that it was not any of these, from Paul through Bloomfield, but all of them, each contributing an increment, none fully accepting the notion but each tending to ascribe it to his predecessors.<42>

10. Granularity. In this section and the next two we deal with some issues that do not bear immediately on our main theme, yet are important as background.

The term that appears as the title of this section had no noticeable currency in the nineteenth century, but for that very reason it can serve us well as a neutral cover label for a number of superficially disparate developments.

Only a few centuries ago, people puzzling about what things are made of sometimes proposed various fluids as among the ultimate constituents. There are everyday fluids like water, air, blood, and breath, which we still class as such and on whose familiar behavior the concept was based. There were also diverse "subtle fluids," such as heat, under the pseudonym of phlogiston or caloric, or aether, proposed as the all-pervading medium whose vibrations constitute light, or the mysterious, sour-tasting electricity (still nicknamed "juice"). An obvious property of fluids is that they flow. But that is not really the defining characteristic, since so does sand, which is not a fluid but a bunch of small, hard pieces that can slide past one another. In contrast, a true fluid seemed to be indefinitely divisible. No matter how small a drop is, you can split it into smaller drops that retain all the fluidity of the larger one.

In the 1770s, when alchemy was transmuted into chemistry with the discovery of oxygen, hydrogen, and nitrogen and the concomitant banishment of caloric (by Henry Cavendish (1731-1810), Joseph Priestley (1733-1804), Carl Wilhelm Scheele (1741-1786), Antoine Laurent Lavoisier (1743-1794), and a handful of others), it began to be suspected that fluids such as water or air might, after all, be like sand, except that the constituent grains are very, very, very much smaller. Even solids might have similar fine structure; after all, sand can be pressed into sandstone. This notion was really not new; the ancients had entertained it (e.g., Democritus of Abdera, c460-c370 BC). But for the modern world it was first put on a persuasive basis by John Dalton (1766-1827), who in 1803 presented his conception of material atoms--Democritus's word--tied up with a theory of atomic weights that to this day remains a cornerstone of chemical theory.<43>

In 1811, independently of Dalton and approaching the problem along other lines, Amedeo Avogadro (1776-1856) introduced a different granular theory of matter; his grains were molecules. It was not until 1858 that this notion, as reworked by Stanislaw Cannizzaro (1826-1910), was

Sprachforschung" (Leipzig, 1901). Quickly summarized by Bloomfield in op. cit., p. 18. Discussed in detail, quoting key excerpts, by E. A. Esper, Mentalism and Objectivism in Linguistics (New York, 1968), chapter 1.

42 I must note here also my feeling that in his generally superb summary of the history of linguistics (op. cit., chapter 1) Bloomfield gave rather shorter shrift than he should have to what probably deserves to be called a third tradition: the diligent, often fruitless, but sometimes superb work of European Christian missionaries in all parts of the world. Such highly practical synchronic work is hardly to be identified with the philosophical speculations of a Mundt. But its impact was great: for example, the Standard Alphabet of Lepsius (first edition 1856) was prepared and published at the behest of missionaries, through their societies, and incorporated a great many of the results of their observations; its influence on subsequent phonetic research is well known.

43 Bernard Jaffe (1896-1944), Crucibles: The Story of Chemistry (New York: Simon and Schuster, 1930; Dover reprint, revised and enlarged, 1976) is written in much too journalistic a style, but the essential facts and dates seem entirely reliable. This section also draws once more on Williams and Steffens, op. cit. (supra, fn. 10).

shown to be not only compatible with Dalton's atomic theory but a necessary supplement to it (molecules are built of atoms, much as written words are built of letters, though with much more complex geometry). Still a third sort of required small unit, the ion (required to explain the behavior of solutions), was introduced by Svante Arrhenius (1859-1927) in 1883-4.

These complications seemed at first to leave Dalton's atoms as the smallest units; atoms had properties, but not constituents. But there were difficulties, compounded in the 1890s by the discovery of radioactivity by Antoine Henri Becquerel (1852-1908) and by Marja Skłodowska Curie (1867-1934) and her husband Pierre (1859-1906). After the turn of the century these difficulties led to a total revision of atomic theory, much of it by Joseph John Thomson (1856-1940), Ernest Rutherford (1871-1937), and Henry Moseley (1887-1915). The atom, despite the etymology of its name, was clearly no longer to be viewed as the smallest constituent of matter, but as a structure built out of even smaller particles. At first there seemed to be only two such, the electron and the proton, but we all know what has happened to that simple notion in the ensuing decades.

Meanwhile, thirty or forty years after the announcement of Dalton's theory, a similar idea came to the fore in biology in the form of the granules--rather squishy ones, to be sure--called cells. The physiology of Bichat and his immediate successors, which we mentioned in section 3, was exclusively a physiology of tissues and juices. Cells had been spotted, but their detailed observation was prohibited by the poor state of microscopy. The major difficulty, chromatic aberration, was overcome early in the 1830s by the genius of Giovanni Battista Amici (1784-1863); at about the same time, new and more effective stains for slides were introduced. With these new tools, cellular structure appeared everywhere. In the late 1830s the botanist Matthias Jakob Schleiden (1804-1881) and the physiologist Theodor Schwann proposed that the cell is the basic physiological building block, the elementary unit of which all tissues are composed. Schleiden, by the way, was a vitalist, convinced that cells are in themselves inert, animated by a separate life force, whereas his collaborator believed that all the behavior of a cell is the result of its own physical structure. Yet they agreed in their announced theory, including its only major feature that subsequently proved erroneous: namely, that cells are born from a "nutrient soup" (a fluid!). It was Rudolph Virchow (1821-1902) who, in the 1840s, rejected this notion, proposing instead that cells arise only from other cells.

Nineteenth-century physics was, in an interesting way, a partial hold-out against granularity. Throughout the preceding hundred years and more, because of the enormous prestige of Isaac Newton (1642-1727), a granular (corpuscular) theory of light, seemingly implied by Newton's optical experiments, had held sway, at the expense of the almost equally convincing wave theory of Christiaan Huygens (1629-1695). At about the turn of the nineteenth century, crucial new experiments, first by Thomas Young (1773-1829) and then by Augustin Jean Fresnel (1788-1827), tipped the scales, so that the wave theory, as reshaped especially by Fresnel, reigned supreme. Only with the quanta of Max Planck (1858-1947), Albert Einstein (1879-1953), and--eventually--many others, beginning with Planck's publication of 1900, did granularity come back: radiant energy behaves in many ways like waves, but it comes in packets that can't be unpacked.

In mathematics the opposition of fluidity and granularity appears in the guise of the continuous versus the discrete. The latter comes from arithmetic, in which the positive integers are granular building-blocks par excellence. The former, the continuum of real numbers (including both rationals like 2 or 2.2 and irrationals like the square root of 2), comes historically from geometry, where the "ratios" of lengths, areas, or volumes are often not at all "rational." (It is no accident, of course, that ratio, reason, and rational in all its senses, and their equivalents in the other languages of modern Europe, all trace back to the same Latin etyma: the association of the two meanings was maintained in Medieval Latin and was then transferred to the modern languages as they came to be used for learned purposes.) The pressure toward granularity in nineteenth-century thinking manifested itself among mathematicians as a profound logico-emotional malaise about the continuum, which was crucial for vast areas of pure and applied mathematics but seemed to have no valid basis. H. Charles R. Méray (1835-1911) expressed this by insisting that the real numbers cannot simply be accepted as an intuition from

geometry; they must be a "creation of the human intellect." That meant one had to start with something logically impeccable, like the integers, and, step by step, build the elements of the continuum. How is one to attain the irrational by rational means? How can one synthesize a fluid from granules? But it was done. Méray was one of a remarkable cluster of mid-century mathematicians who worked on this and related issues. One solution was foreshadowed by Augustin-Louis Cauchy (1789-1867) and carried to fruition in the early 1870s independently by Méray and by Karl Weierstrass (1815-1897); another, the most elegant and simple of all, was rather diffidently proposed in 1872 by Julius Wilhelm Richard Dedekind (1831-1916).⁴⁴

To see the connection between the above and our own field we must go back to antiquity.

Some of the early Greek mathematicians—for example, Pythagoras of Samos (c580-500 BC)—were firmly of the opinion that irrational numbers do not exist. They believed, rather—in the face of persuasive geometric evidence to the contrary—that there must be some smallest quantity of which all other quantities are integral multiples. [If this rejection of irrationals seems naive, we should recall that even in the nineteenth century, in the times of Méray, Weierstrass, and Dedekind, there were creative mathematicians who took the same position: for example, Leopold Kronecker (1823-1891).] Now, although the surviving evidence for Greek thought is probably much too scant for us ever to be sure, it is not improper to suspect that the material atomism of Democritus, to which we made passing reference a few paragraphs back, was derived from this slightly earlier quantitative atomism.

But if Democritus's material granularity came from Pythagoras's abstract quantitative granularity, whence the latter?

It seems reasonable to propose that, whatever other influences there may have been, one factor promoting this style of thinking was literacy—the literacy of the Greeks in an alphabetical writing system. The most casual glance at Greek inscriptions reveals letters as discrete successive units (even if they often touch). Texts in such a system supply an excellent paradigm of granularity. What is more, the fact that alphabetical writing works proves beyond challenge (as Bloomfield and others have pointed out) that speech itself, however continuous it may sound, is for its human users fundamentally granular; that is the first and, I believe, the most important proposition of the phonemic theory. Given the crucial role of talking and writing in human meditation, and given, especially, the Greek belief in a very close tie between language and "mental" phenomena, it would not be at all surprising for this fundamental feature of the design of language to serve as a constant out-of-awareness model as they looked into the nature of other things.

I see no reason not to assume that language, and alphabetical writing, played just this same supporting role in the granular theories of the nineteenth century.

Here, then, we have the reverse of the situation dealt with in earlier parts of this essay. In general, ways of thinking prominent in the learned world as a whole have influenced the theorizing of linguists. But in this case workers with language had the pattern first.

11. Patterning. It may be true no longer, but when I was a sophomore sophomores still argued whether a triangle is equal to or greater than the sum of its parts. What makes this question sophomoric is that both answers are not simply wrong but stupid: to take the issue seriously is to try to apply arithmetic where only geometry will do. A triangle, obviously, is not a sum but an arrangement of its parts. Replace all the parts and you still have a triangle. If two figures are both triangles, it is because, in both, the arrangement of parts manifests the pattern known as triangularity; they need share nothing else. In course of time every constituent atom of a person's body is replaced, but the pattern persists and continuity

44 Carl B. Boyer (1906-1976), A History of Mathematics (New York: John Wiley & Sons, 1968). For the "Dedekind cut" see pp. 607-8; a fuller and more formal treatment in Sir Godfrey Harold Hardy (1877-1947), Pure Mathematics (Cambridge: 1908, 7th edition 1938), sections 1-8.

of identity is unbroken.

In our culture we seem to prefer to conceptualize patterns in spatial terms whenever possible. Benjamin Lee Whorf (1897-1941) told us that this habit is not a human universal; the Hopi, for example, do not have it.⁴⁵ Perhaps literacy has played a part here, too, in the constant exposure to the two-dimensional surfaces on which we write (and draw), in contrast with the three spatial dimensions in which we live and move about. Think, thus, of a graph in a physics book in which a patterned relation between two totally nonspatial things, temperature and pressure, is presented via the two spatial dimensions of the surface of the page. Our habit in such matters is very useful; the fact that it is not a cultural universal does not make it bad.

The pattern of granularity that we discussed in the preceding section manifested its influence as people in various fields were looking for the smaller constituents of which larger things are made.

In chemistry, the search led to atoms and also to elements, which is to say, atoms of different kinds. But it was obvious early on that a knowledge of the constituents is not enough. One also needs to know the proportions in which they are combined, and the arrangement--the pattern, here clearly spatial--of the parts relative to one another. You can't make an arbitrary quantity of hydrogen (an arbitrary number of hydrogen atoms) form a compound with an equally arbitrary amount of oxygen. If there are twice as many hydrogen atoms as oxygen atoms, the resulting compound is water. If the numbers are the same, with some difficulty you get hydrogen peroxide. In any other proportion, some atoms of one or the other element are left out. An accumulation of empirical facts of this sort led at mid-century to the formulation of the theory of valence: an atom of hydrogen will combine with only one other atom (has a valence of one); an atom of oxygen will combine with just two other atoms; a carbon atom has a valence of four. One of the architects of the valence theory was Edward Frankland (1825-1899), who introduced the term in 1852; the clarifications of Cannizzaro in 1858 also helped pave the way. The next step was to think about spatial arrangement. For example, given the known valences of hydrogen and carbon, how is it possible for six atoms of each to combine to form a molecule, as they do in benzene? In 1865, Friedrich August Kekulé von Stradowitz (1829-1876; like Frankland, he had spent some time with Liebig in Giessen) suggested that the carbon atoms were joined together in a ring, alternately with single and double bonds, each bonded also to a single hydrogen atom.

It cannot be irrelevant that before turning to chemistry Kekulé had been a student of architecture.

The climax came in the 1870s (miraculous decade!). In 1874, the Dutch chemist Jakob Henrik van't Hoff (1852-1911) and the French chemist Joseph A. Le Bel (1847-1930) independently proposed that the behavior of carbon and its compounds could be understood if one assumed that the carbon atom has its four valences at the corners of a tetrahedron. The following year, 1875--the year of Charles Lyell's death, of the composition of the epic Fifth Symphony of Anton Bruckner (1824-1896), and of the appearance of "Eine Ausnahme der ersten Lautverschiebung" by Karl Verner (1846-1896) and of Whitney's The Life and Growth of Language--van't Hoff published La chimie dans l'espace, and structural chemistry was here to stay.

Meanwhile, over in the other great moiety of Wissenschaft, granularity in the form of "elementalism" still held sway--at least in psychology. (Actually, that is not quite right--that

45 "The Relation of Habitual Thought and Behavior to Language," in Leslie Spier (1893-1961), ed., Language, Culture, and Personality: Essays in Memory of Edward Sapir (Menasha WI: Sapir Memorial Publications Fund, 1941), pp. 75-93; reprinted in John B. Carroll, ed., Language, Thought, and Reality: Selected Writings of Benjamin Lee Whorf (Cambridge MA: The Technology Press, 1956), pp. 134-59. Some of Whorf's other essays on Hopi, included in the Carroll volume, also touch on the issue.

is, the implied assignment of psychology is not, since the field had come to be split between the two moieties, with physiological psychology obviously part of natural science but with still great effort to deal directly with mental matters.) Herbart's psychology was very much an attempt to break psychic phenomena down into their ultimate, atomlike constituents. Steinthal, in adapting Herbart's scheme to the treatment of language, changed details but did not modify the basic orientation, nor did Paul in the psychological frame of reference of his *Prinzipien* of 1880. In the great Delbrück-Wundt debate to which we referred in section 9, the differences between the Herbartian psychology presented by Delbrück and the Wundtian alternative clearly seemed vital to the participants; yet both views were indubitably elementalistic (or "analytic"), the major difference being which mental features were viewed as primary and which as emergents. (This was not the main point at issue in the debate, but it is the one that concerns us here.) Thus, for example, (46)

a *Vorstellung* ['idea'], which is for Herbart a psychical element, is for Wundt a compound composed predominantly of sensations. A feeling, which Herbart derives from the relations between ideas, is for Wundt an unanalyzable element.

I have to remind myself forcibly from time to time that, however all this may strike us (or me) today, in the intellectual climate of the time these fine scholars were not talking nonsense. To be sure, no one could directly observe the mental phenomena that concerned them, in the way one can observe, say, a swinging pendulum or a sweating palm. But no one can see an atom either. The chain of reasoning from what one actually can weigh and measure in the laboratory to the chemist's conceptualization of atoms, molecules, valence bonds, and the like is long and tortuous--to the extent that some extreme positivists, like Ernst Mach (1858-1916), were led to reject the whole business. Given a hardly challenged physical-mental dualism, it surely made sense to try strategies in mental science analogous to those so successful in physical, with no reason to expect chains of inference to be any less twisted in the one than in the other. A chemical experiment is performed with substances in test tubes. A psychological experiment, given the mental nature of what is being investigated, has to be conducted differently; for example--a procedure developed especially by Wundt's student Edward Bradford Titchener (1867-1927)--by introspection, or by having one's experimental subjects introspect and report what they find. But it is an experiment just the same. And we should not forget that it was Wundt who, in 1879 at Leipzig, founded the world's first psychological laboratory.

As for the domination of elementalism in psychology, the patterning antidote came along eventually, but only several decades after the triumph of the structural view in chemistry. It appeared in the form of what for many years was known by the special label "Gestalt psychology"; if the term is not heard much any more, that is because its most important features have been absorbed into the mainstream of the discipline. Of course, there had been many foreshadowings--if one wanted to one could even trace these back to Plato (c428-c347 BC), whose term *idéa* meant not 'idea' but 'form (versus substance), pattern, Gestalt'. Whether the development of structural chemistry had any influence on the thinking of the earliest Gestalt psychologists I do not know, but I would find it hard to believe that it did not. In 1890, Christian von Ehrenfels (1859-1932) issued a germinal article, "Über Gestaltsqualitäten," presenting the German word in its special sense for the first time. (47) But he carried the matter no farther; that was left to the trio who deserve to be known as the founders of the field: Max Wertheimer (1880-1943), Kurt Koffka (1886-1941), and Wolfgang Köhler (1887-1967). Wertheimer's foundation article was published in 1912--just the year in which appeared the last, definitive, but still resolutely elementalistic edition of Wundt's *Die Sprache*.

46 E. A. Esper, op. cit. (supra, fn. 41), p. 17.

47 *Vierteljahrschrift für wissenschaftliche Philosophie*, vol. 14.

The Gestaltists worked first with perception, and the burden of their sermon was just my burden at the beginning of this section: quite apart from its constituents, a figure such as a triangle has a pattern, a Gestalt, which is what makes it a triangle. Perceptually this example is a matter of vision, or perhaps of touch. But the same principle applies for other sensory modalities. If you whistle "Dixie" you ain't just whistlin' "Dixie," because you also have to choose a tempo, a volume, and a pitch range (key and register); yet within limits all of these can be modified without rendering the tune unrecognizable. What is it, then, that constitutes the true essence of "Dixie"--the distinctive features of the tune, to use a familiar linguistic term--as over against the inconstant and nondistinctive yet ineradicable features of tempo, volume, and pitch range? The essence is a pattern of arrangement of the parts relative to one another. Can we take away all the parts (without replacement) and still have the pattern, as the Cheshire cat's grin lingered when the cat itself vanished? Plato thought so; in fact, for him the abstract forms were the only ultimate reality, their exemplifications but shadows on the wall of the cave. If you accept a physical-mental dualism or if, like Plato, you are a monistic idealist, I guess you can agree with that. In any case, this is not a point on which Gestalt psychologists have been of an undivided opinion.

Gestalt is related to what we linguists call "hierarchical structure," but is not the same thing. A sentence consists of clauses, which consist of phrases, which consist of words, which consist of--well, morphemes or phonemes or sounds, depending on your angle of approach. A metazoan organism (e.g., a jellyfish or a person) consists of organ systems, which consist of organs, which are built of tissues, which are built of cells, and so on. But you don't get an organ by just listing its constituent tissues; you have to arrange them. Humpty-Dumpty knew this; he found Alice's physiognomy dull because

"your face is the same as everybody has--the two eyes, so--" (marking their places in the air with his thumb) "nose in the middle, mouth under. It's always the same. Now if you had the two eyes on the same side of the nose, for instance,--or the mouth at the top--that would be some help."

12. Pattern in linguistics. In syntax, Gestalt plays such a crucial role that if one takes it away there is nothing left--and this has been so from the very beginnings of the discipline in classical antiquity. The doctrine which views a sentence as a whole composed of an arrangement of suitable parts is set forth elegantly, in plain speech aimed at schoolboys, by Whitney in the following passage; the doctrine itself is at least as old as Apollonius the Grouch (Al-exandria, 2nd century A.D.)⁽⁴⁸⁾

This name, "part of speech," given to a word, plainly implies that there is something incomplete about it; that it is not a whole, but must be joined with other "parts" in order to make a whole, or in order to be speech.

That is in fact the case; and the whole which these parts make up is the sentence.

All our speech, as we actually use it in talking or writing, is in sentences; we do not really say anything unless we make a sentence.

If, for example, we speak the words sun, tree, ink, goodness, he, we are only mentioning something; any one who hears us will naturally ask, "Well; what about it?"

So, too, if we say the, with, golden, brightly, away, tall.

But if we say

48 Essentials of English Grammar for the Use of Schools (Boston: Ginn & Company, 1877; 2nd edition 1886), pp. 7-8. (The handset typography of the original is extremely elaborate; I have had to simplify it here.)

the sun shines; the tree is tall;
he writes with ink; they went away;

we have really said something. It may be very uninteresting; it may be foolish; it may even be false; but it is at any rate something said; the person to whom we speak does not need to wait for it to be finished in order to approve or reject it. We have thought something and said it; we have made up our mind to some purpose or other and told what it is; we have (as it is called) formed an opinion or judgment, and expressed it by a sentence.

A [declarative] sentence is, then, in the sense thus explained, the expression of a judgment.

So, at least in syntax, nineteenth-century linguistics not only needed no helping hand from chemistry or psychology but could, and perhaps did, supply subtle assistance in the other direction.⁽⁴⁹⁾ But two points must be appended.

The first point is that, because of the mentalistic terms with which all syntacticians operated before 1900, it is not always easy to know whether a particular discussion is intended to treat of language or of the operation of the mind. Even accepting a dualistic frame of reference, there is danger here of circularity. In the case of kinds of words, Whitney escapes the trap by insisting that nouns, verbs, and the like must be defined not semantically or philosophically but in terms of the work they do in sentences. But then his definition of declarative sentence turns on the mental act of making a judgment. That explains the linguistic concept only if one already understands the psychological one. Antoine Meillet (1866-1946) finally solved this problem in 1903 with the operational definition of "sentence" that Bloomfield subsequently seized on with such joy.⁽⁵⁰⁾ But now, suppose Whitney's definition (or equivalent) is presented to someone who is perfectly familiar with declarative sentences but not at all sure what is meant by a "judgment." In such a case, does not the linguistic frame of reference serve to define the psychological concept? And that leads in turn to the really disturbing question: to what extent were Wundt and Delbrück and their predecessors talking about the mind, and to what extent were they simply talking about language in a rather indirect and awkward way?

The other point to be appended is that nineteenth-century descriptive grammars (synchronic, and almost always pedagogical or practical), though in some cases including long and detailed discussions of syntax, also in many cases had little or none. I don't know why Whitney's Sanskrit Grammar (1879) contains no syntax, but it was certainly not because he frowned on the topic: perhaps he simply lacked the time, or perhaps he felt that Sanskrit syntax is sufficiently like that of Latin and Greek that most users of his book would not need the help. The latter was a common reason for slighting syntax without in the slightest scorning it. For example, in 1830 Rask published a grammar aimed at speakers of English who wanted to learn Danish, and the section on syntax is brief for the avowed reason that in this respect Danish and

49 Through what channel? Recall that in the period we are talking about, although only some scholars studied chemistry or psychology, all started out with thorough exposure to Latin and Greek, using school grammars.

50 The definition is in the first edition of Meillet's Introduction à l'étude comparative des langues Indo-Européennes (Paris: Hachette, 1903), in the first paragraph of the chapter on the sentence (p. 355 in the 8th edition, 1937); Bloomfield adopts it in "A Set of Postulates for the Science of Language," Language 2.153-154 (1926), fn. 4, though he words it wrongly in his Definition 27; he corrects the wording in Language 7.204-9 (1931), fn. 6, and, of course, in his 1933 book.

English are not very different.<51> There may have been some tendency in the nineteenth century to underestimate syntactic differences (especially in the case of "exotic" languages like Chinese or Choctaw), perhaps reinforced by a conviction that except in minor mechanical details the laws of syntax at larger size-levels are just the "laws of thought" and hence universal. But in fact it does seem genuinely the case, as Bloomfield pointed out,<52> that languages differ more at lower size-levels (e.g., derivation and inflection) than at higher (e.g., the combining of clauses), so that the consequences of the attitude were not inevitably disastrous. Indo-European comparative study, true enough, after the early fiasco of Schleicher's 1868 reconstruction of a whole myth,<53> sensibly put off detailed inferences about ancestral syntax until there was a sufficiently broad and reliable base to support them. But delay is not neglect.

However, when we come to Saussure the story is different. From the available evidence, it appears that Saussure considered syntax and the sentence to belong to "parole" rather than to "langue." That was a motivated exclusion (or, at least, allocation), and apparently, because of Saussure's entirely deserved prestige, his attitude engendered unprofitable views among some of his followers. I think Saussure may here have been misled by his own frame of reference. His contrast of langue and parole has always seemed to me to be a confusion of two distinct oppositions: (1) the social versus the individual, and (2) habits, institutions, patterns, ways of behaving, versus events, actions, behavior. When we remember that at the turn of the century some scholars were still speaking quite seriously not merely of mind versus matter for individuals but also of "group mind" or "the collective soul" or the like, it is perhaps not surprising that even an original, creative, and empirically oriented thinker should have melded habit and the social on the one hand, behavior and the individual on the other.<54> But in fact habits are necessarily a matter of the individual organism--except in the sense that many people may have the "same" habit just as many plane figures can all have triangularity. Even if I am wrong in that (which I may be if, after all, ontological dualism is true), the separation requires to be made; Saussure's terms cover too much at once. Having made the separation, we can see that although any actually uttered sentence, as a historic event, is indeed parole (=behavior), it achieves sentencehood by being built by one of the patterns for sentences in the particular language. And patterns for sentences are surely no less a part of langue than are patterns for such smaller assemblies as words or word-parts.

In phonology the picture has been more mixed.

In Grimm's day there was as yet no very convenient terminology for distinguishing between writing and speech. A word, as a sort of "molecule," was viewed as composed of letters as "atoms," but the letters could then be interpreted, depending on context, either as graphic shapes or as sounds. (Our more recent usages should not lead us to misunderstand Grimm's heading "Über den Buchstaben"; he and his contemporary readers knew he was talking about sounds, not about marks on paper.) In the 1860s Whitney used the entirely unambiguous expression "letters

51 A Grammar of the Danish Language for the Use of Englishmen, by Erasmus Rask [sic; but it's our guy] (Copenhagen: J. H. Schultz, 1830).

52 His Language, p. 207.

53 Reprinted as a curiosity by Jespersen, *op. cit.* (supra, fn. 14), pp. 51-52.

54 It is true that Saussure knew the work of Émile Durkheim (1858-1917), whose discussion of the conscience collective of a society can easily be read as proposing a soul-body dualism for social groups parallel to that for individuals. But in Saussure's own work (as reported) I find no faintest suggestion of an acceptance of such a notion, except just for the specific confusion I have mentioned. On this I think Geoffrey Sampson, in his earnest but too hastily written Schools of Linguistics (London: Hutchinson & Co., 1980, chapter 2) is quite mistaken. Saussure's notions are equally compatible with the competing--and nonmystical--theories of Gabriel Tarde (1843-1904), for whose book Les Lois d'Imitation (1890) Hanns Dertel (1868-1952) expresses such gratitude in his 1901 Lectures on the Study of Language (New York: Scribner's).

of the spoken alphabet." Some time after Grimm's earliest work--I don't know just when--German-speaking linguists developed the convenient label Sprachlaut. In 1873, when A. Dufriche-Desgenettes introduced phonème, he intended it merely as a handy French equivalent of that German term, in place of the awkward phrase une voyelle ou une consonne.^{<55>} That is also the only sense in which Saussure ever meant the new word.

The significance of the preceding is that, as the phonemic principle gradually emerged, the model for it, beyond all shadow of a doubt, was traditional alphabetic orthography. The immediate stimulus for the principle, however, was (as first pointed out by Twaddell in 1935^{<56>}) the discovery of phonetics.

We can see this in the mid-nineteenth-century work of Richard Lepsius, Isaac Pitman (1813-1897), Alexander J. Ellis (1814-1890), and Alexander Melville Bell (1819-1905), and in the slightly later researches of Henry Sweet (1845-1912), L'Abbé Pierre Jean Rousselot (1846-1924), Eduard Sievers (1858-1932), Paul Passy (1859-1940), and Otto Jespersen.^{<57>} The earlier investigators sought to develop regular and consistent orthographies for languages that had none, and better writing systems for languages, such as English, that had traditional systems full of irregularities--the latter at least for various special uses, such as teaching the language to foreigners, to children, or to the deaf, if not in all cases as a matter of spelling-reform. At first, working with languages they knew well, all seemed to be clear sailing: it is simple enough to replace the c of can by k and the ç of çyst by s, and to make other adjustments of that sort, even if new letters have to be invented. But as they observed articulation more and more closely, and as they took more and more languages into consideration, the number of sounds they could discriminate and the number of symbols needed to denote them both increased seemingly without limit. With the development of instrumental phonetics, largely by Rousselot, it soon turned out that, given sufficiently accurate measuring devices, there really is no limit. But even before that the known variety had become disturbingly great.

The phoneticians had thus--unexpectedly--uncovered an enormous mass of important data. But that was not what they had originally set out to do. Traditional alphabetical orthography, even if not freed from all the inconsistencies that had crept in with the passage of time, seemed to depict the words of a language in a simpler and more straightforward way than did a close-grained phonetic transcription salted with strange new letters or peppered with diacritics. In speech the delivery of a word may involve all manner of delicate details of sound, but the bulk of these are either transient (different from one occurrence of the word to another) or predictable from the relatively few features that are both constant and distinctive. And so the phoneticians began to develop notations that would show only those constant and distinctive features and let the rest be provided for by cover statements. In this way, the difference between phonetics and phonemics slowly (and painfully) emerged. Some of Sweet's predecessors never grasped the difference, but most of his contemporaries and successors did, in one fashion or another. Sweet himself had the essence of the phonemic principle (without Dufriche's new word) at least by 1877, the year in which he introduced the adjective distinctive in the sense in which we still use it.^{<58>}

I am not saying that phonetics is inevitably elementalistic and that phonemics can blithely be equated with Gestalt. In a general way, phonetic research demands a concentration on indi-

55 Report of the meeting of 24 May 1873 of the Société de Linguistique de Paris, in Revue Critique d'Histoire et de Littérature 7.368 (1873).

56 William Freeman Twaddell (1906-1982), On Defining the Phoneme (Language Monograph No. 16, 1935), second paragraph.

57 The work of some of these scholars is reviewed, from one angle, in Robert William Albright, The International Phonetic Alphabet: Its Backgrounds and Development (Publication 7 of the Indiana University Research Center in Anthropology, Folklore, and Linguistics, 1958).

58 Handbook of Phonetics (Oxford: Clarendon Press), sections 302-304.

vidual sounds and their classification, which is certainly elementalistic, but that need not preclude paying attention also to how the sounds are arranged and how they affect one another in the flow of speech; thus in Sweet's Handbook the section on "Analysis" is followed by an almost equally long section on "Synthesis." On the other hand, to interpret the sound system of a language phonemically demands the active (if not always overt) recognition of words, phrases, or utterances as holistic units in which the constituent distinctive features are arranged in patterned ways. For example, to assert that the difference between the light-colored lateral of British English leaf and the dark one of feel is not distinctive, or that the two sounds are "the same phoneme," is to imply that what the two have in common is a (recurrent) constituent and that the differences are functions of the arrangement. If that implication is not there, the assertions are meaningless.<59>

Let us turn now to the three most prominent traditions of phonemic theory in the second quarter of the present century. We know their prime initiators: Daniel Jones (1881-1967) and in some measure Harold E. Palmer (1877-1949) in England; Franz Boas (1858-1942), Edward Sapir (1884-1939), and Bloomfield in the United States; Nikolaj Sergevic Trubetzkoy (1890-1930) and Roman Jakobson (1896-1982) in Prague. Of these three, it seems to me that the British--except for the remarkably original ideas of John Rupert Firth (1890-1960)--showed the least systematic response to Gestalt and that the Americans showed the most. I would like to know if any of the Prague theoreticians were actively influenced by the Gestalt psychologists. I am reasonably sure that none of the earliest American workers were. Yet both Praguian and post-Bloomfieldian American linguistic theory have been dubbed "structural," and the structure of a thing, event, or system, if I understand it aright, is nothing other than the pattern it manifests. Moreover, in the middle of the present century there was--and perhaps still is--a whole complicated "structuralist" movement, in fields as diverse as ethnology, literary criticism, and mathematics, some of whose participants proclaim their methodological indebtedness to Prague.<60>

Actually, Praguian and American structuralism in phonology were quite different. Prague emphasized the make-up of individual (letter-sized) phonemes, somewhat to the neglect of pattern-

59 Excessive elementalism still creeps from time to time into what poses as phonemic research. A mere table of the phonemes of a language affords little useful information, because so much depends on higher size-levels of organization (syllable part, syllable, articulatory phrase, and so on) and on the privileges of occurrence of smaller units within larger ones. Therefore part 5, "Languages of the World," in Merritt Ruhlen's A Guide to the Languages of the World (1975, released through the Language Universals Project at Stanford University), despite the diligence that went into its preparation, is, regrettably, of limited value.

60 For an excellent survey of this movement, see Jean Piaget (1896-1980), Le Structuralisme (Paris: Presses Universitaires de France, 1968); English translation Structuralism (New York: Basic Books, 1970; Harper Torchbook, 1971).

The anthropologist Claude Levi-Strauss says his way of thinking in cultural analysis derives from Prague phonology: see his "L'Analyse structurale en linguistique et anthropologie," Mot 1.1-21 (1945); or a revision of that as the second chapter of his Anthropologie structurale (Paris: Plon, 1958); or an English translation of the chapter, "Structural Analysis in Linguistics and Anthropology," in Dell Hymes, ed., Language in Culture and Society (New York: Harper & Row, 1964), pp. 40-52. I find Levi-Strauss's speculations stimulating, and I do not doubt the earnestness of his extension of credit to Prague, but in fact I see no significant resemblance between his notions and phonemic theory of any brand (Praguian or other), nor does it seem to me that his ideas in any special way qualify as "structural." It may be that the structuralists mean something by the term quite different from my expectations. In any case, I fear that "structural" has become a conjure word--especially, perhaps, in literary criticism.

To bring structuralism to mathematics is coals to Newcastle, for mathematics is the science of structure in the most general and abstract sense.

ing in larger units; Bloomfield, on the other hand, considered the phonetic details of phonemes to be of only practical importance, not part of scientific linguistics, but dealt in detail with phonemic arrangement. Prague looked for pattern in tabular arrangements of phonemes--as it were, on the "axis of simultaneity" (Saussure's "paradigmatics," Bloomfield's "selection"); America looked for patterning in the actual flow of speech (the "axis of successivity," Saussure's "syntagmatics"). For example, confronted by varieties of German in which the earlier apical trill *r* (as in *rein*) has become a voiced dorso-velar spirant, the Praguian approach immediately wants to interpret it as the voiced partner of the voiceless dorso-velar spirant of *Bach*; the Americanist approach notes that this new *r* does not have anything like the distribution of the other voiced spirants (*y* and *z*, as in *Wein* and *sein*) and prefers therefore to keep it paired with *l* (as in *Lein*), many of whose privileges of occurrence it does share.

Each of the two schools did well with what it paid attention to, and each was wrong about what it neglected. By the 1940s many of us were realizing that, and trying to integrate the two approaches. It will probably not surprise anyone that in my opinion the culmination of this, so far, is the whole-to-part immediate-constituent orientation presented in my Manual of Phonology of 1955.⁶¹ But I must immediately add that I am convinced the full impact of the Gestalt view has not yet been felt. If we can learn to take that approach in a consistent way, I believe many of the problems that beleaguered us in the 1930s, 1940s, and 1950s (and that have been largely neglected between then and now) will turn out either to be spurious or to have simple and satisfying solutions.

13. The Fate of Laplacean Determinism. Our winding trail now leads us back toward the Laplacean doctrine with which we began. But if we are to understand what happened to it we must pause en route for several brief panoramic views.

(1) Karl Marx (end of section 5 above) was by no means the only investigator of his times to look for the factors that control human destiny. Of the many others we will mention only two. In America, the sociologist William Graham Sumner (1840-1910), a defender rather than an opponent of capitalism, taught that it just does not matter what individuals try to accomplish because what actually happens is entirely determined by impersonal social forces.⁶² Somewhat later, the German sociologist Max Weber (1864-1920), fascinated yet troubled by Marx's ideas, came to conclusions differing from Marx's but equally constraining on our supposed free will.⁶³

More important in the long run was the slow discovery of human cultural transmission or tradition: the passing down of patterns from generation to generation by learning and teaching. Expanding Europe gave scholars more and more information about peoples whose lifeways were simply not comprehensible in European terms. Preeminent among those who came to grips with this and sought explanation of the variety in terms of culture were Adolf Bastian (1826-1905), Lewis Henry Morgan (1818-1881), and Edward Burnett Tylor (1832-1917).⁶⁴ Tylor's first major work, Researches into the Early History of Mankind and the Development of Civilization, was published in London in 1865, and Whitney may have known it, since even in his first book (1867) he uses

⁶¹ Indiana University Publications in Anthropology and Linguistics, Memoir 11 (Supplement to International Journal of American Linguistics vol. 20).

⁶² Two of Sumner's essays, "Sociology," originally published in 1881, and "The Absurd Effort to Make the World Over," from 1894, are conveniently reprinted Perry Miller, op. cit. fn. 26.

⁶³ "Die protestantische Ethik und der 'Geist' des Kapitalismus," Archiv für Sozialwissenschaft und Statistik 20.1 & 21.1, 1904-5; English translation, The Protestant Ethic and the Spirit of Capitalism, (New York: Scribner, 1930).

⁶⁴ For an excellent survey of the work of these founders of anthropology, see Robert Harry Lowie (1883-1957), The History of Ethnological Theory (New York: Farrar & Rinehart, 1937).

the term "culture" in exactly the sense the early anthropologists were giving it. Two points concern us here. One is that culture constrains free will: the goals people seek and the means to their ends that are likely to occur to them are both strongly conditioned by their heritage. The other is that cultural transmission is a channel of causality, a mechanism of continuity and change, for human ways of acting and thinking.

There is, of course, another channel, one shared by all forms of life on earth: genetics. The results of the pioneer work of Gregor Johann Mendel (1822-1884), announced in 1865, though lost from sight for a number of decades, were rediscovered in 1900 by Hugo de Vries (1848-1935) and his coworkers. By the 1920s Mendel's "factors" had been renamed genes, and Thomas Hunt Morgan (1866-1945) had demonstrated that genes are actual physical entities, segments of chromosomes in the nuclei of cells. Since then investigators have accumulated remarkably detailed information about the molecular structure and the action of genetic material. We are people rather than chimpanzees because about 1% of our genes are different from those of chimpanzees; we vary among ourselves because, of that remaining 1%, no two of us (except one-egg twins) have exactly the same assortment.

The struggle to get genetics and tradition recognized as the mechanisms of transmission of human lifeways, displacing vague and often mystical or prejudicial notions of "race," "national character," or "collective soul," was a long and difficult one. Help came from many quarters, by no means just from anthropologists; for example, from such French sociologists as Gabriel Tarde.⁶⁵ As late as 1908, when Boas submitted the manuscript of the first volume of the Handbook of American Indian Languages to the Bureau of American Ethnology,⁶⁶ he was impelled to begin its Introduction with a strong reminder that race (that is, physical type), language, and culture are all different, working themselves out independently, or with minimal interaction, in human populations. It seems to me that Whitney already knew this, but there were plenty of scholars in his times who did not. Nor did Boas's words settle the issue; we all know that fanciful racial theories are still to be found. And even though the genetic and cultural mechanisms are now reasonably well understood, the problem of their interplay remains, despite the signal contributions of such great scholars as Theodosius Dobzhansky (1900-1975).⁶⁷

That is our first panorama. The moral--lest there be any doubt about it--is that in the course of the hundred years that followed Laplace the clarity of the contrast between matter (with determinism reigning supreme) and mind (with free will) was considerably muddled as investigators uncovered more and more causality in the mental world. The climax was capped by Sigmund Freud (1856-1939). Coming to the examination of human conduct from an angle very different from those of the economists, sociologists, ethnologists, and geneticists, he uncovered intimate lines of causality in the lives of individual human beings of a sort almost wholly unsuspected by his predecessors. Some have thought that he was insufficiently aware of human diversity, his perspective confined to the ways of life of turn-of-the-century Vienna. But Vienna was not itself exactly monolithic, and he made use of its internal cultural diversity in his work. In any case, the point of importance for us is that, in imitation of the methods of Na-

⁶⁵ See supra fn. 54.

⁶⁶ Published as Part I of Bulletin 40 of the Bureau of American Ethnology, Smithsonian Institution (Washington: Government Printing Office, 1911). The Introduction was reprinted some time in the 1960s (no date given), with a rather silly "timely" Foreword, by the Institute of Languages and Linguistics of Georgetown University. For the various racial theories of the late nineteenth century as they influenced the development of anthropological thinking, see Lowie, op. cit. supra fn. 64.

⁶⁷ See especially his Mankind Evolving: The Evolution of the Human Species (New Haven CT: Yale University Press, 1962).

turwissenschaft, he based his thinking on an unshakeable assumption of strict psychic causality; without that he could not have reached the conclusions he did.<68>

(2) In the 1920s "cultural relativity" was rendered fashionable in anthropology by the work of Boas and some of his students, especially Ruth Fulton Benedict (1887-1948). Although the doctrine itself did not derive in any serious way from physics, there is no question but that the label was seized on because it had been made prominent by Einstein. The unfortunate thing about this superficial similarity is that it can conceal from us a much deeper and more important shared feature.

In Tylor's day one of the great issues in anthropology was the problem of resemblance through diffusion versus resemblance through parallelism. The fast accumulating body of information about human lifeways all over the world was revealing many cases of similar phenomena in widely separated places. A famous example is the couvade, a custom whereby the husband must take to bed when the wife delivers a child. This appears among the Basques of the Pyrenees and recurs in aboriginal South America. In any such case it may be either that the custom originated in a single place and diffused to the sites where it has been observed, or that it was invented independently two or more times.<69> For a long time, investigators sought general principles of some sort which could be applied to solve individual cases correctly. It has turned out that there really aren't any such principles--other than the tautology (sometimes concealed by fancy wording) that if one of the two explanations is impossible, the other must be right.

But at least there are some useful guidelines. One of these is that geographically separated similarities can be the result of diffusion from a single source only if enough time has elapsed for the trait to be carried from a point of origin to the places of attestation. Diffusion is the movement of a trait through space, but, in order to move, either the trait has to be carried by people as they migrate or it has to spread by imitation from tribe to neighboring tribe, and both of these take time. That is why specialists are almost certain that the aboriginal writing systems of the New World, which first appear in or near Yucatan towards the end of the second or the beginning of the first millennium BC, must be indigenous, rather than derived from those of the Old World that began only about two thousand years earlier. The principle involved here can be phrased aphoristically: space is time.

Einstein's special theory of relativity, announced in 1905 (the year of Jules Verne's death), is--or includes--the cosmic equivalent of the purely earthly principle we have just stated.<70> More than three centuries ago, in 1676, the Danish astronomer Ole Roemer (1644-

68 For an example of Freud's awareness of cultural differences, see his General Introduction to Psychoanalysis (lectures delivered at the University of Vienna in 1915-1917; English translation by Joan Riviere, London, G. Allen & Unwin, 1922; American Edition, Boni & Liveright, 1924), Lecture 22, paragraphs 23 and 24.

69 The same problem appears in evolutionary theory, where the wings of birds and the forelimbs of mammals are called homologous because they are the outcome of different lines of development from the same anatomical feature of a remote common ancestor, whereas the wings of birds and of insects are only analogous, evolutionarily unrelated structures performing the same function. For the historical development of the diffusion-parallelism issue in anthropology, see Lowie, op. cit. supra fn. 64.

70 Einstein's original paper, "Zur Elektrodynamik bewegter Körper," Annalen der Physik 17 (1905), is more straightforward and easier to understand than most of the numerous popularizations that have appeared since. An English translation of that paper, together with English versions of a number of other of the original contributions to the theory of relativity, was published as The Principle of Relativity (London: Methuen, 1923; also a subsequent undated Dover reprint).

1710) determined that (A) Light travels at a finite speed. Einstein built his theory on that well known fact, and also on the work of James Clerk Maxwell (1831-1879), Albert Abraham Michelson (1852-1931), Edward Williams Morley (1838-1923), Hendrik Antoon Lorentz (1853-1928), and George Francis FitzGerald (1851-1901). The features of the theory that concern us, in addition to point (A) above, are these: (B) The speed of light (in a vacuum) is a constant; measurements of it come out the same no matter how the measuring apparatus is moving relative to the source of the light. (C) That speed is an absolute speed limit, attainable only by radiation (not by any material body), and hence also an absolute speed limit for the transmission of information. And so--as every schoolchild knows--when we look through a telescope at the great galaxy in Andromeda we see it not as it is but as it was 2,000,000 years ago, because it is about 2,000,000 light-years from us. As is the worldly case, space is time.

(3) Our third view en route--really too brief to be called a panorama--has to do with the quantum theory. That is what the statistical mechanics of the nineteenth century, mentioned above toward the end of section 5 (along with the atomic theory of Thomson, Rutherford, and Moseley touched on in section 10), was transformed into in the early 1900s by the work of Planck, Einstein, and a remarkable coterie of theoretical physicists of which we shall name only Niels Henrik David Bohr (1885-1962), Louis Victor Pierre Raymond de Broglie, Wolfgang Pauli (1900-1958), and Werner Karl Heisenberg (1901-1976).⁷¹

Along with relativity, the quantum theory has already revolutionized physics--nor is the end of the revolution yet in sight. But of all its fascinating findings, we need only the following: It is not possible to measure both the position and the momentum of an object with complete precision. To increase the accuracy of the determination of position, it is necessary to sacrifice a degree of precision in the concomitant measurement of momentum, and vice versa. For large objects, such as stars or speeding bullets, the effect of this indeterminacy is so small relative to the magnitudes of the quantities being measured that it is not detectable. But for, say, an electron, the effect is, relatively speaking, enormous.

This indeterminacy cannot be circumvented by improved techniques, because any measurement necessarily involves a physical interaction between the measuring apparatus and the measured object, and it is just this interaction that yields the indeterminacy. It does not arise from human failings or from poorly designed experimental methods; it is built into the fabric of the universe.

Einstein's special relativity theory was announced in 1905, Heisenberg's uncertainty principle in 1927. Laplace's doctrine of strict physical determinism therewith became untenable.

A Laplace's demon cannot exist.

In the first place, such an omniscient entity is required to know all the forces operative in the universe "at a given instant." But under relativity there is no such thing as a "given instant"--a "universal 'now'" in the sense assumed in Newtonian physics. There are only a now here and a now there, and if the demon is now here it can know what is happening now there only in the future, after enough time has elapsed for information to travel from there to here. Space is time.⁷²

In the second place, for the demon to know even the forces operating on a single particle it must measure the values of six variables, three of position and three of momentum; but under

⁷¹ See d'Abro, op. cit. supra fn. 20.

⁷² One might imagine the demon being omnipresent as well as omniscient. But then the information gathered everywhere "simultaneously" has to be brought together internally for processing, and the relativistic limitation again appears. We could also imagine the demon somehow standing outside the physical world and not being subject to its constraints, but if we do that we can imagine anything, with no way to test our hypotheses.

quantum indeterminacy the more precisely one set of three is determined the less precise must be the measurement of the other three. The exact measurements demanded of the demon are physically impossible.⁷³

Laplace's reasoning told him that the physical world is absolutely determinate and that our inability to make absolutely accurate predictions is a function of our inability to make absolutely precise measurements (and absolutely exact calculations). The truth seems to be a slightly skewed opposite of that. What needs explaining is not why we cannot predict more accurately, but why we are able to predict at all. The comportment of the tiny little constituents of the universe is, within fuzzy limits, random and unmeasurable. Determinacy and predictability appear only statistically, in the mass. So, after all, Marx's kind of determinism is the only kind there is.

In a century, then, the basis for the Great Dichotomy, so obvious and unquestionable at the outset, had become considerably obscured. On the one hand (our first panorama above) the notion of free will had been subjected to a deadly bombardment by a series of dedicated investigators of human conduct. On the other, the idea of absolute determinism in the physical world had been absolutely refuted.

That does not mean, of course, that the ontological doctrine of mind-body dualism had been shown to be wrong. It means only that virtually all of the traditionally accepted evidence and arguments in support of it had become, and still today remain, untenable.

14. **Bloomfield's Physicalism.** For many of us, the work of Leonard Bloomfield constitutes the high point, so far, in the history of our discipline. Certainly anyone who has examined the classics of linguistics with care will agree that his *Language*, published now just fifty years ago, stands head and shoulders above all its predecessors; nor does any general work done since then hold a candle to it. In prose poetic for its simplicity, Bloomfield integrated almost all that had gone before, with a perceptivity and realism that derived also from his own creative work in virtually every type of linguistic research.

Bloomfield developed his mature views about the psychology of language, after an earlier period in which he had accepted Wundt's psychology as the best available, largely under the influence of Albert Paul Weiss (1879-1931), his colleague at The Ohio State University from 1921 through 1927. Weiss had been the student of Max Meyer (1873-1967), whose own apprenticeship had been served in Germany; the earlier pedigree of this scholarly line has been traced for us by Weiss's student E. A. Esper.⁷⁴ At the outset, Bloomfield, like Weiss, called his approach "behaviorist." By the mid 1930s, having learned something of the work of the logical positivists of the "Vienna Circle,"⁷⁵ he came to prefer their term "physicalist," but only as a better label for essentially the same orientation.

73 For a different (though equivalent) explanation of the impossibility of a Laplace's demon see Richard von Mises (1883-1953) *Probability, Statistics, and Truth* (originally in German: J. Springer, 1928; third German edition, 1951, translated into English and published 1957 by George Allen & Unwin Ltd.; Dover reprint 1981), Sixth Lecture.

74 Esper, op. cit. supra fn. 41. Weiss's crucial book was *A Theoretical Basis of Human Behavior* (Columbus OH: R. G. Adams, 1925; 2nd edition 1929). Meyer wrote the fascinating *Psychology of the Other One* (Columbia MO: Missouri Book Co., 1921).

A few years ago Martin Joos (1907-1978) expressed to me the suspicion that a more personal factor may have played some part in Bloomfield's seemingly radical shift of orientation in the early 1920s. His household in Columbus included his sister Marie, born about 1905, but at the time an honor student in economics at Barnard College. In February, 1923, she committed suicide.

75 The leaders of this group were Otto Neurath (1882-1945) and Rudolf Carnap (1891-1970); the latter came to the University of Chicago in 1936 (Bloomfield was there from 1927 to 1940).

A good deal of Bloomfield's stance--not quite all of it--can be set forth in the following three propositions:<76>

(1) Human language behavior (indeed, human communicative behavior of all sorts) can be fully accounted for in terms of physics and biology, properly developed and extended, without positing any nonmaterial or "mental" entities.

(2) Such aspects of human conduct as belief systems, artistic endeavors, ethics, law, science, and mathematics, obvious realities and yet difficult to dissect and explain, can also be understood monistically--provided we allow for the unique role of language.

(3) Conversely, however: a linguistically naive monism or physicalism is as impotent as any dualism, for it cannot explain the differences between human behavior and that of other animals.

To these must be added at least two other points: (4) a rejection of teleological explanations, and (5) a refusal to make any use of reports of introspection.

In Whitney's day such an approach would not have been reasonable, since it would have entailed the denial of various obvious realities. But, as our earlier discussion has shown, the intellectual context of linguistic theory had altered considerably in the intervening decades, so that by the 1920s an orientation of this sort made sense. To be sure, some of Bloomfield's contemporaries accused him and Weiss (and other behaviorists) of just such denials, but on careful scrutiny it turns out that their accusations were built largely on careless reading.

Nevertheless, there were some obscurities and confusions in the Weiss-Bloomfield position, as well as some features sensible at the time but rendered untenable by subsequent developments, and it is these that I wish to discuss.

First: Bloomfield and Weiss seem not to have realized that an insistence on physical monism, in contrast to traditional mind-body dualism, is not the same thing as a denial of the reality of internal mental activity. Nor is either of these the same as a refusal to consider introspective reports evidentially relevant. One can reject Titchener's psychology (as the behaviorists did) simply on the grounds that evidence must be public, without at all impugning the introspective experience. And quite independently of one's views about that, one can hold that mental activity is a reality without implying any nonphysical mind or soul: one's view can be that it is a complex epiphenomenon in a purely physical universe.

In a sense, this last is what Bloomfield and Weiss proposed, except that in the line-up of things that had to be accounted for in physicalist terms they replaced "mental" activity by language activity; that, rather than thinking, was to be the bridge between the physical-biological understructure and the human social world. On this, more in a moment.

Having insisted on the three-way distinction that Bloomfield and Weiss did not make, I must now add that to my own way of thinking the only really crucial ingredient in this part of their position was the physicalism. Whether or not introspective evidence is to be used is a question of methodology, and we have need of many methods. And whether one speaks of mental activity in traditional terms, or paraphrases in order to avoid unwanted dualistic implications, is largely a matter of terminology, rhetoric, and individual taste.<77>

76 The first three points are reworded from p. xiv of my brief biography of Esper in Esper, op. cit. supra fn. 22. (I now consider inadequate some parts of that treatment of Bloomfield's behaviorism.)

77 I really doubt that Sapir was, at bottom, any less a physicalist than Bloomfield; yet he spoke freely in what Bloomfield frowned on as "mentalistic" terms. It should be noted that although the two has the highest respect for each other, they disagreed, in a good-humored way,

Second: Tangled up with the foregoing is another confusion, a melding of two different oppositions reminiscent of the case we found in Saussure's work (see above, section 12). To spell this out I shall have to use two everyday words, for the nonce, as technical terms. (Let me make it clear that in doing so I in no way imply that other uses of the words are in any way defective. It is the distinction I am about to describe that counts, not the wording.)

By thought (versus action) I shall mean internal mental activity--imagining, dreaming, implicit trial-and-error, talking silently to oneself, playing Beethoven's Fifth Symphony privately in one's head, and so on. Thinking is an individual matter. Maybe nonhuman animals do it too; that seems likely from circumstantial evidence, but they cannot report their introspections as our fellow human beings can. By theory (versus practice) I shall mean communicative behavior, talking things over, consulting about problems, and other such essentially social activities (though, to be sure, single individuals can also use their repertoires of communicative skills in communing with themselves, either silently--which is also thinking--or aloud--which is acting). To paraphrase an example of Bloomfield's: when the town council, sitting around a table, considers a report of a damaged bridge and issues orders for it to be repaired, they are engaged in theorizing, which will lead to the practical activity of the crew that does the work. Each member of the council may think some things that don't get said aloud (as well as some things that do); each member of the work crew may think in terms of body movements and tool manipulation before actually performing the operations that repair the bridge (and may dream about it the following night).

We see that the thought-action opposition and the theory-practice opposition are mutually orthogonal. It is true that theorizing and the communicative systems that make it possible are to human social groups what thinking and the central nervous system are to the individual. But that is a similarity, not an identity, and Bloomfield and Weiss made trouble for themselves (as have many others) by ignoring the distinction.

Third: There is a minor but potentially confusing inconsistency between Bloomfield's use of the word "language" in his technical linguistic writings and the use of the same word in his more philosophical discussions. On the one hand, in harmony with practically all the linguists of his times, he insists that only speech is language: writing is a derivative secondary system, just as are gestures, smoke signals, whistle talk, and so on. But then in speaking more broadly of the role of language in human life he implies, without ever making it absolutely clear, that he means not only language in the narrow sense but also all these diverse derivative systems. In my phrasing, above, of point (1) of his orientation, I allowed myself to be rather more explicit about this than he himself ever was, by including, after "language," the parenthetical remark "indeed, human communicative behavior of all sorts."

Although minor, the discrepancy is not completely trivial, because it can be misleading. In fact, I know of one scholar (not to be identified here, of course) who seems genuinely to believe that language in Bloomfield's narrow sense is the only bridge we need between physics and biology on the one hand, love and honor and patriotism and music and politics and science on the other. So for the more inclusive category we do better to speak not of language but of communication--or even, perhaps, since virtually everything passed down in the human world by learning and teaching seems to serve some communicative function, simply to go along with the anthropologists and speak of culture.

Fourth: Bloomfield's rejection of teleological explanations probably reflects his exposure to botany while an undergraduate at Harvard. Anyone who studied botany in the early decades of

about the sorts of things we are here discussing: in private conversation (according to Carl F. Voegelin, *viva voce*), Sapir called Bloomfield's psychology "naive," while Bloomfield characterized Sapir as a medicine-man.

this century was taught very sternly never to say "the flower turns toward the sun because it needs the sunshine," or anything of that ilk. Explanations had to be phrased in terms of causes, not purposes. Once Bloomfield became convinced that dualistic psychology was wrong, so that the study of language is just as much a branch of Naturwissenschaft as the study of plants, teleology obviously had to be eradicated from the one as much as from the other.

The inference was sound at the time, but is no longer. Of course, it would be nonsense to propose finalistic explanations in the original sense which Aristotle (384-322 BC) seems to have intended: by definition, a later state or event cannot be the cause of an earlier one. But there exist mechanisms--a furnace with a thermostatic control is one--that "pursue a goal." How such a device works is well understood, and there seems to be no doubt that the organic world is full of this sort of circuitry. In this sense there certainly is such a thing as "goal-seeking behavior," so that to speak of it in circumstances where the necessary kinds of hookups exist is not to abandon the path of scientific rectitude.<78>

And finally: There is some problem as to just how Bloomfield's physicalism related to his conception of causality and determinism. It does not seem likely that in the early 1920s he could have known of the radical weakening of the notion of determinism in the physical world that was just then taking place. The implications of special relativity were there, to be sure, for anyone who worked them out. But Heisenberg's uncertainty principle, the keynote of the quantum indeterminacy of which we have spoken, was announced only in 1927. Even after that, in 1933, Bloomfield penned passages that sound to me as though he still accepted something very much like Laplacean determinism--in the physical world and therefore, for him as a physicalist, in the world. Let me quote from his book (p. 32):

The mechanism which governs speech must be very complex and delicate. Even if we know a great deal about a speaker and about the immediate stimuli which are acting upon him, we usually cannot predict whether he will speak or what he will say.

I have always heard this as a bit regretful: if we knew enough, we should be able to predict more.<79> That my perception is right is confirmed by the way he spells out the physicalist orientation on the next page.

So the physicalism of Weiss and Bloomfield actual came before some of the discoveries in other branches of science that were most crucial in rendering their view tenable. I am not quite sure what to make of that. Surely the new attitudes were "in the air" by the early 1920s. Yet perhaps in being ahead of schedule they were also--at the time--wrong: they had constructed a mansion with no foundations, and it was largely serendipity that others came along and supplied the needed underpinnings before it toppled.

15. Conclusion. In ending my account at this point I by no means intend to imply that nothing of importance has happened since 1933. Indeed, we have already had occasion (in the latter part of section 11) to refer at least briefly to the "post-Bloomfieldians," who carried

78 The proper book to cite here is probably Norbert Wiener (1894-1964), Cybernetics (New York: The Technology Press and John Wiley & Sons, 1948), though both psychologists and engineers had already been talking about servomechanisms for several decades.

79 As for the quoted passage, it should also now be pointed out that if we did know enough about a speaker and about the immediate stimuli to be able to predict what was going to be said, the speaker would not need to say it! As Claude L. Shannon told us a decade and a half after Bloomfield's book in his Mathematical Theory of Communication (Urbana IL: The University of Illinois Press, 1949), what is communicated to us is information only if we didn't already know it. This emphasizes again the lesson we have drawn from relativity: space is time.

certain parts of linguistics considerably beyond the Bloomfieldian horizon. Also, factual data on languages all over the world has accumulated at an unprecedented rate, and of an average quality far above that of the last century; for any empirical-minded linguist (which is what we all should be), that is of great importance. In addition, a number of new trends or "schools" of linguistic theory have arisen, some of them more prominent than others and one of them vastly more prominent than it deserves to be, all of which are supplying food for thought--supplying it, indeed, rather faster than anyone can possibly ponder it.

I do mean to imply that, in my view, there have been no developments either in linguistics or in the scientific world as a whole demanding any major revision of the Bloomfieldian physicalist orientation discussed in the preceding section (the revisions suggested there, though important, are minor).

In the years after the Second World War we have seen a resurgence of philosophical dualism, a twist on eighteenth-century rationalism that I think would best be dubbed "logical dualism." This doctrine (or one form of it) teaches that in addition to the physical universe there is, not a moral or mental realm in the old sense, but a logical universe, in which logicians can make discoveries just as physical scientists make them in the physical world. Something like this seems to be the view of certain contemporary British philosophers, such as P. E. Strawson.⁽⁸⁰⁾ Then whatever may seem to be true about language and grammar in the physical world, at bottom they belong to the logical universe, deriving from logic itself.

This is all reminiscent of Wundt's insistence on the possibility of discovering a psychology of language totally unrelated to its history, to the point of positing evolutionary sequences that have some sort of abstract psychological validity even though historically they are nonsense.⁽⁸¹⁾ And it is relevant for us because the underlying philosophical orientation of transformational-generative theory is a version of the new dualism, and the popularity of TG, though not easy to account for, is a fact of life that has to be faced. We cannot simply say that the new dualism is false, since, like Plato's idealism, it might actually be true. The difficulty with any such possibility is the same as the difficulty, mentioned in footnote 72, of imagining an omnipresent demon not subject to the laws of physics: any hypothesis of that sort is useless, because there is no conceivable way of proving it false. It would be pleasant to be able to predict that in due time this new philosophy will fade, giving way once more to approaches worthy of being called scientific. But we have no grounds for such optimism. At least, though, we can point out that logical dualism is totally out of keeping with any scientific findings of the last two centuries. Instead of being intricately intertwined with the latest trends in many different branches of scholarship, it stands alone, supported by nothing but faith. I prefer to close ranks with my fellow scientists.

⁸⁰ See his Subject and Predicate in Logic and Grammar (London: Methuen & Co., Ltd., 1974). I hardly need add that to me this view is a reversal of the truth, derived from a failure to distinguish properly between symbol and symbolized. What these people mean by "logic" is in my view an unrefined mixture of two ingredients: (1) some very general facts about the universe that are part of the fabric of the physical universe, and (2) some specialized ways of speaking (or writing), with rules to prevent the introduction of nonsense where it is not wanted.

⁸¹ See Esper, op. cit. supra fn. 41, pp. 38-9.

INAUGURAL LECTURE

WHAT IS SYNTAX?

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If we want titles to be attractive, we should make them short and snappy. Mine is short and I hope it is attractive. But it is not quite indicative of the way I am approaching the problem of syntax. Actually it should have been : 'What should we call syntax?'. I hope it is clear by now that in scientific matters we cannot and should not use traditional terms without redefining them. If we really want to know what syntax is, we should try to determine how people in general use that term. But since we know that it is used very loosely and that the various usages cannot be reduced to a common denominator, the only approach I would recommend is trying to find a definition or better, a stipulation that will make syntax perfectly distinct from the other disciplines needed for the analysis and presentation of the significant units of a language.

I am confident most people in this audience have definite ideas about what syntax is or should be. I owe them an apology for not starting with a presentation and discussion of their views. But that could certainly take us too far afield. I shall have to restrict myself to the presentation of my own views on the subject with the hope that a discussion will follow where conflicting opinions will be given a chance. Yet, I shall begin with a criticism of what I think is a very common conception of syntax. I suppose that if we asked point blank a number of people with a minimum of schooling what syntax is, a majority would answer that it is the study of how words combine in utterances. This is not too bad as a starting point, and we can for a while concentrate on the terms 'word' and 'combine' in order to determine what each of them implies and why we shall not retain them when we try to delimitate the field of syntax.

It is interesting to notice that most linguists, even among those who are prone to discard traditional views and notions, hesitate to get rid of the concept of 'word'. It is true that the term is very handy and, in the present instances, I have to watch myself for not using it instead of the innocuous 'term'. But it should be clear that even in scientific discourse we may, at times, use plain everyday language if context, situation or voice inflexion indicate that we are not,

at that moment, making use of a scientific terminology.

For more than fifty years, progressive linguists have been using specific terms for the designation of minimal significant units. Among them, 'morpheme' has the widest currency on both sides of the Atlantic. But it has definite disadvantages : first, there is, in some quarters, a lingering tradition according to which 'morpheme' designates a minimal grammatical unit in contradistinction to 'semanteme' - as if grammatical units were meaningless ! - and more recently to 'lexeme' ; second, even among those who use 'morpheme' for both grammatical and lexical units, there is no agreement as to what is formally needed for positing a morpheme : in Europe, most linguists would say that complementary distribution suffices to establish the identity of a morpheme, so that the -en of oxen is the same morpheme as the -es of horses.

You know better than I what the answer would be at this date in the United States. But when, in the summer of '59, I asked my colleagues at Ann Arbor, Michigan, what they thought about that problem, I found a whole range of opinions ; but, generally speaking, they thought some formal likeness was needed for identifying a morpheme : cows, horses and goats would share a sibilant morpheme, but, in oxen and children, we would have to posit some other unit. In view of this lack of agreement among the linguists of the world, I thought it preferable to use another term for the designation of the minimal significant unit and adopted 'moneme', a term proposed by Genevan Henri Frei. Later on, I discovered that Frei himself had what was, to my mind, a far too formal conception of the moneme. But by that time it was too late for me to make another choice.

The chief objection to dropping all references to form when defining the minimal significant unit is that we could then easily end up with calling monemes what is usually identified as features of meaning. If we analyse went as 'go' + 'past', why shouldn't we analyse mare as 'horse' + 'female'? The answer is that went and mare are not on a par : went is what is called in Europe a syntagm, i.e., a grammatical phrase corresponding to two different choices by the speaker ; mare, just like princess or she-goat, represents a single choice. We need not use single choice as a criterion. What really counts in that no part of mare, princess or she-goat can be determined independantly of the rest. Syntactically it is immaterial whether commutation is possible, as in princess or she-goat, or not as in mare. Princess and she-goat are lexical phrases or, as we call them, synthemes. A single choice unit need not be analysed any further unless the analysis is obvious as in the case of princess or she-goat. All of this belongs to a compara-

tively new comprehensive chapter of linguistic description, called 'synthematics', including composition, derivation, and the gradual fusing of free combinations into syntheses. Since what is linguistically important, in the case of syntheses, is their behaving syntactically like monemes, there is no reason to analyse a form like business where the original elements are amalgamated, or blackmail where the formal duality does not give a synchronic clue to the meaning of the whole. We have of course to distinguish between joint monemes, as in princess and she-goat and free monemes, as in went and Latin plural genitive rosarum, just as in showed or of the roses. Here, of course, 'free' implies 'free choice', and not, as in the Bloomfieldian tradition, formal independence.

We are now, at long last, reverting to the notion of 'word' and the question is : if we operate with a minimal significant unit, do we need a more complex one between the moneme and the sentence? I, for one, don't think we do, at least as long we are not discussing the written form of some languages. In the presentation of the oral form of French which is found in our Grammaire fonctionnelle du français (Paris, Didier, 1979) no use is made of 'word', various groupings of monemes being referred to as syntagms, i.e., combinations of monemes that are more closely related among themselves than with the rest of the utterance. Sentences are accordingly syntagms, but ones that have no syntactic connections with the rest.

If we try to see what sort of syntagms would get closer to what is referred to as 'words' in lay parlance, we notice they are usually made up of a lexical nucleus with the addition of determinants with or without some connective. The determinants in question are not determinable : in the big man, big is a determinant of man, but it is determinable, as in very big, and consequently does not belong here ; the, on the contrary, is not determinable and, therefore, would qualify here but for a restriction that is to come. Undeterminable determinants are conventionally called modalities. Connectives can be defined as monemes whose appearance depends on the existence of two other units, monemes or segments larger than monemes. If the conditions delineated so far should suffice to qualify a phrase as a word, with the men would be a word. But, of course, it does not qualify, one of the fundamental reasons being that three of the constituting items can be separated from the rest by insertions, as in with all the big men? This is usually the case when modalities and connectives precede the lexical nucleus, but not when they follow it, as in Danish manden 'the man', Romanian nasul 'the nose', English noses, or under the form of case endings often

combining the connective (the case itself) and the indication of number as in Latin rosarum 'of (the) roses'. With the help of some Greek friends, I coined a name for such a complex, namely 'syllemma'. Unfortunately, we cannot use the term 'word' for it, because it often does not tally with general usage, as, for instance, when some prefixed modality is tagged directly to the nucleus, such as the augment in Greek élabon 'I took', to lambáno 'I take'. But, of course, the syllemma is little but a joke rhyming neatly with dilemma, and we never need it when we describe a language. Its ephemeral existence is just meant to emphasize the impossibility of making, in the case of 'word', lay usage tally with the needs of a scientific approach to the science of language.

If we do away with the notion of word, a very widely diffused conception of morphology will have to be reconsidered. If we asked a number of people who have had some grammatical teaching at school how they understand the term morphology, most of them would probably say that morphology is the part of grammar that deals with words by themselves, as opposed to syntax, which deals with their combinations. If we now drop 'words', what should morphology amount to? What we actually find, as an introduction to morphology in traditional grammars, is a sketchy listing of the grammatical constituents of the word, for instance, in Latin, a mention of the names of cases, with just hints as to their values - this being dealt with at greater length in the so-called syntax of cases - and a reminder of the existence of two numbers, singular and plural. The bulk of morphology consists in a presentation of various forms assumed by the different endings, corresponding to our modalities and connectives, with mention of how the stem behaves when in contact with the various additional items. This is made under the form of declensions and conjugations, which are probably the best way of dealing with what is, from a logical standpoint, a terrible mess, the one our XIXth century predecessors used to be so proud of, the so-called inflections.

In our practice, what corresponds to the lists of word-constituents is not distinct from the treatment of the various classes of determinants, whether they appear before or after the nuclei. The determinants that characterize certain nuclei will be presented as specific classes and the nuclei themselves will be grouped according to their compatibilities with certain classes of determinants. All of this will be dealt with in a chapter called 'inventory'.

The essentials of what is traditionally treated under the heading 'morphology' will remain there, but will no longer be presented as the formal antics of

so-called 'words'. Each moneme will be considered in its own rights in the part devoted to its class, the Latin accusative for instance, being presented as marked by -m in the singular with some neuter exceptions. The declensional framework will be retained in the section reserved for nouns. Morphology will then no longer stand in direct opposition to syntax. On the way that leads from the smallest, distinctive, units of phonology, to the complete utterance called the sentence, morphology will be nothing but the presentation of a number of accidents (remember its still common designation as 'accidence') which have to be memorized by whoever - child or foreigner - wants to use the language to the satisfaction of his audience. As far as the satisfaction of communicative needs is concerned, those accidents will only make it more difficult, since they will impose in specific contexts, different forms for what has, in all cases, the same informative value. At this point, some people will wonder how such a deplorable state of affairs could endure. The answer is that it does not ... in the long run. And if it takes such a long time - millennia - for a language to get rid of morphological nuisances, it is because there can be no planning : among the competing forms of one and the same moneme, analogy will, from one minute to another, favor this or that one, and, of course, grown-ups and schoolteachers will constantly intervene in the defense of tradition. Morphological errors are ridiculed because they immediately strike the audience as socially deviant, and that plays a great role in the preservation of traditional forms. A wrong choice among meaningful possibilities can only be detected by the hearers in reference to context or situation, and by that time, no one will think of making fun of the speaker.

We have so far been concerned with the elimination of the term 'word' from what should be our definition of syntax. Before we tackle the second term in question, namely 'combine', it may be advisable to summarize the conclusions arrived at before regarding the units which could be said to combine. We have, of course, the minimal significant units we call the monemes ; next, we have the syntemes, shown by commutation to consist of two or more significant parts, but behaving with regard to the context just like monemes : and finally we have grammatical phrases, or syntagms, made up of a nucleus, its determinants and, if need be, a connective. As a rule, the relation of a syntagm to the rest of the utterance is identified with that of its nucleus : in I saw the big man, man, and not the big man, is connected with saw. Yet, if an utterance begins with strangely enough, this is likely to determine the following statement as a whole and not just the predication.

ve center as strangely does in he acts strangely.

'Combining' is of course putting together ; etymologically, syntax is 'ordering or ranging together', which is not so different. But the real issue is whether this ordering together is relevant for the interpretation of what is said. More precisely, when two linguistic items are found in contact, three possibilities have to be considered :

First, their contiguity is a pure matter of chance : in I saw him there, him and there are in direct contact, but both are determinants of saw and are not directly related ; if there had been placed before I saw, the relations between the different items would not have been different.

Second, the contiguity is just a matter of habit : you say big man, not man big ; if some foreigner said man big, every body could understand that the relation of big to man would still be one of determination, man remaining the nucleus. Native speakers would be tempted to make fun of it. The respective positions of big and man are part of the morphology of the language.

Third, the respective positions of the two elements in question is fully relevant since inverting them would change the message : the succession ... the cat kills ... indicates that the cat is the killer ; the succession ... kills the cat ... that the cat is the victim. In English, the respective positions of verb and noun are relevant and, for that language, it makes sense to speak of an SVO pattern. But, in the case of Latin where the subject is marked as such by the nominative case and the object by the accusative, positing SOV is at best indicative of a preferential order that belongs to stylistics rather than syntax. Using S, V, and O as a basis for the establishment of a syntactic typology is totally aberrant since the respective positions of the items so designated may be irrelevant, not to mention the fact that the notions of verb, subject, and object do not make sense in a number of languages. This deserves to be remembered as a beautiful example of linguistic ethnocentrism.

What is decisive in syntactic matters, is not combinations, but compatibilities, i.e., the possibility for the monemes of a certain class to coexist with those of another class in a definite relation. Syntax is, properly, the study of such relations.

The necessity of syntax stands in direct relation to the vocal nature of human language. If man had chosen to communicate by means of visual signals likely to endure, such as pictures, or readily combinable, such as gestures, it would not be reduced to a linear succession of ephemeral noises. Human experience, what language is intended to communicate, is not by nature linear.

Prior to its confrontation with the resources of a given language, it is likely to be a nebula rather than a complex of well-ordered notions. Its analysis into elements corresponding to the monemes of the language entails the choice of some connecting items or relevant orders that will enable the hearer to reconstruct the experience in its pluridimensionality from the linear succession of speech. The aim of syntax is to extract and order all the features that contribute to turn a succession of distinct units into a coherent whole.

In many cases, the units, by themselves, imply the type of relation they entertain with immediate, or mediate, neighbors : the, as such, is known to be the determinant of a unit know, in its turn, to be a noun, that is one likely to be accompanied and determined by the or some moneme of the same class. But in other cases the exact nature of the relation between monemes know to be compatible remains to be shown. Traditionally, it is said that, e.g., a noun may assume different functions in its relation to a verb. In practice, syntax will consist in investigating these different functions, simple unambiguous determinations being taken care of in the inventory. But, of course, no one could object to considering the inventory as the first chapter of syntax.

Traditionally the only grammatical functions that are treated at any length are the statistically frequent ones as, for instance, the subject function which is, as it were by definition, present in all self-sustaining utterances. Among frequent functions we have the ones that are found only in combination with certain verbs, the object-function, for instance, which characterizes the so-called transitive verbs, or the dative function which may accompany verbs implying a transfer toward a beneficiary, etc. Their expression often entails morphological complications, such as the Spanish alternation of a and zero for the object or, in English, that of to and zero for the dative.

Most other functions do not depend for their appearance on the presence of a given verb : any state or action may be conceived of as located somewhere, as taking place at a certain time or in a certain fashion. In such cases, the function is often expressed by some particle which is the same in all cases and consequently does not raise any morphological problem. All of this accounts for the fact that many grammarians think that for a language like English or French, a listing of the different prepositions is all that is needed. Here again, people are far too prone to conceive of the presentation of formal accidents as the sole aim of grammar.

Although I am convinced that all grammatical functions should be given their share of attention, it

cannot be denied that, the world and mankind being what they are, the functions involving the participants of an action are more decisive than circumstantial ones when it comes to characterizing the syntactic core of the language.

In such matters, linguists, with a modicum of sophistication, distinguish between two basic types : the accusative type and the ergative type. In the terms most of them would use, 'accusative languages' use the same form for the subject of intransitive verbs and the subject of transitive verbs, the object being marked as the 'accusative', 'ergative languages' use the same form for the subject of intransitive verbs and, strangely enough, the object of transitive verbs, the subject being them marked as the 'ergative'. The inevitable conclusion is that 'ergative people' are a queer set.

Now if we cease taking 'subject' and 'object' for granted and operate instead with different participants, the picture becomes notably different : with some verbs (the so-called intransitives), we find a single participant ; with others we have two, the active participant and the inactive participant. The single participant may be active, as in the man is walking, or inactive, as in the man is suffering. In this case the opposition of activity to inactivity is irrelevant.

With this in mind, we may present the problem in different terms : in 'accusative constructions' the active participant has the same form as the single participant, the inactive participant receiving a specific mark, that of the 'accusative' ; in 'ergative constructions' the inactive participant has the same form as the single participant, the active participant receiving a specific mark, that of the 'ergative'. In most cases, the mark of the single participant is zero, that of the 'accusative' and that of the 'ergative' being specific particles or endings. But, in some 'accusative languages' like English, the distinction between single and active on the one hand, inactive on the other hand may be secured by means of specific positions. The same might apply to the parallel distinction in 'ergative languages'.

The distinction between 'accusative' and 'ergative' does not, by any means, exhaust the range of central syntactic patterns. But it has proved instrumental in helping some linguists overcome the belief in the universality of the SOV, SVO patterns. Yet ethnocentrism is so ingrained that the identification of functions by reference to what is found in translations into the language of the describer is still rife. Even those who know better, experience difficulties in discarding the view that 'ergative' is not as natural and legitimate as 'accusative'.

I have found that a good way of getting rid of our prejudice in favor of 'accusative' is to imagine what can take place if we start from a linguistic stage where people are free to use function markers or to omit them. The choice will depend on whether lexical meanings or situation suffice to show who acts and who is acted upon, as in cow-grass-eat, or whether an indication of some sort is needed as in man-tiger-kill. If, for some reason, function marking ceases, in the course of time, to be optional, we may imagine three possibilities :

1. Both active and inactive will be marked, single participation being left unmarked ; if A is the mark of active, and P that of inactive or patient, we shall have, as the case may be, man-A-Tiger-P kill or man-P-Tiger-A kill. This reminds one of the -p and q marks of Eskimo. With single participation, just man-walk, man-suffer.

2. Active will be marked and inactive left unmarked, coinciding formally with single participation which is also left unmarked. We shall have man-A-Tiger-kill or man-Tiger-A-kill. With single participation just man-walk, man-suffer. This is 'ergative'.

3. Inactive will be marked and active left unmarked, coinciding formally with single participation which is also left unmarked. We shall have man-tiger-P or man-P-tiger-kill. With single participation, just man-walk, man-suffer. This is 'accusative'.

In better known languages, the marking of grammatical functions is obligatory and automatic. In Latin, for instance, a noun is never found without a case-ending : there is no speech segment that means just 'wolf' : whatever means 'wolf' in inextricably combined with the expression of its relation to the context, e.g., in lupus, lupum, lupi, etc. In English wolf exists by itself in the dictionary, but as soon as it appears in a context, its relation to that context is shown either by some accompanying preposition (to the wolf, by the wolf) or by its position in the context (the wolf is hungry, keep the wolf away).

Yet, in languages with no case-endings, we come across contexts where the meaning of the noun is the sole indicator of its relation to the rest of the utterance : in English, for instance, tuesday may be heard instead of on tuesday and such phrases as last night or next year carry no indication that they are referring to some period of time apart from what is implied by night and year. In German, the equivalent of 1924 takes no preposition. In French, Boulevard Saint-Michel is understood, by itself, as locating an event. In many African languages, the designation of a forest is normally understood as 'in the forest' since the forest is mainly conceived of as a place where things happen.

This reliance on context, situation, and the meanings of nouns for the indication of function may be carried very far : in many cases in Russian and constantly in Spanish, nouns designating inanimates need no expression of their object function, speakers being free to use position for emphasis if need be. In Burmese, this would seem to extend to all grammatical functions, speakers being free to mark them depending on whether they think their audience need them or not. In qawasqar, a language spoken by the scattered remnants of a native population on the archipelagoes of Southern Chile and recently described by Christos Clairis, apart from a few temporal and spatial connectives, there seems to be only one functional marker, a sibilant, optionally used as the indication of any sort of determination. Cases are on record where two utterances, differing only by its presence or absence, receive the same Spanish translation. Initial position is frequent for the agent, but probably as a mark of optional topicalisation.

If, as I do, we operate with the hypothesis that linguistic evolution largely depends on the changing needs of man, we may posit that the nature of function marking will partially depend upon the structure of the society. When division of labor develops, the society becomes more complex and the types of relation among people more varied ; when communities get larger and cities replace villages, you are likely to meet more people you know little about and whose behavior you cannot foresee.

The evolution of Indo-European languages affords good illustrations of the way a fairly limited pattern of functional indicators is gradually superseded by a more expandable one. The case of Latin is well-known : in its earliest period, when it was still the linguistic medium of the shepherds of Latium, it operated with five syntactic cases. But when Rome developed into a great metropolis it became imperative to give a more precise syntactic rendering of the variety of social relations. This was achieved by eking out traditional case-endings by various adverbs like ad, de, in. These ultimately became prepositions, the meanings of the less specific case-endings getting blurred and the endings themselves getting lots through mergers or phonetic decay. With the pastoral populations who preserved Latin in Dacia under the form of Rumanian, the process was delayed. The communicative needs of shepherds was certainly less varied, the result being that modern Rumanian has a set of prepositions that bears only distant resemblance to that of the Romance languages of the West.

It should be kept in mind that many different factors may interfere with those we have just considered. Japanese affords an interesting illustration of how the

satisfaction of some other need may hamper the elimination of the optionality of functional indication. The two function markers ga and o, which some linguists could be tempted to equate with nominative and accusative, were certainly optional in the old days. They still are in the sense that either of them can be replaced for emphasis by wa.

It is always difficult to decide whether a given linguistic behavior is more economical than some other one. Option, here, implies weighing for or against identifying and expliciting a given relation. Automatic function marking does away with any weighing, but requires, on the part of the speaker, that he identifies every single relation as belonging to a certain type. It would seem that if the necessity of function marking is exceptional, the occasion of weighing for or against will be rare, and option will be more economical. If, on the contrary, that necessity is frequent, automatic marking will spare the necessity of deciding whether the function should be marked or not. Consequently, when social relations become more complex, we may expect automatic marking to spread. But, here again, we should refrain from positing two and only two perfectly distinct types, one totally optional, another totally automatic, even if we have found languages that are fairly representative of these two extremes. Generally speaking, once we have stipulated what syntax should be, we should always remain on the look-out for unexpected syntactic patterns and never let our judgment be warped by fashionable binary mirages.

I have so far avoided some problems, which, to many people in this audience, may appear far more fundamental than the ones I have discussed at some length. One of them is how can we draw a boundary between semantics and syntax. Such a boundary is, to be sure, not always obvious. When, for instance, a certain relation is found to be either explicitly indicated by a connective or suggested by the specific meanings of the lexical terms involved, we may wonder to what extent the latter belongs to syntax. If we decide that syntax is whatever contributes to transcend the linearity of speech, it certainly does belong to it. Yet, since whatever significant value is added to the sum of the lexical ones is found to be identical with what is contributed by the connective when it is used, we may say that it only figures in syntax in so far as it shares the value of the syntactic tool, the connective.

Those who find it difficult to keep syntax and lexical semantics apart are, as a rule, the ones who make grammatical functions intervene in the establishment of monemes classes. It should be clear that if we decide that the possibility for a given unit to assum

this or that function is used promiscuously with the compatibilities of lexical monemes and modalities in order to achieve our sorting, we shall end up with a swarm of classes, each one consisting of very few units. Considering verbs, for instance, we would have, from the start, to distinguish transitives and intransitives, verbs that accept dative function and the ones that do not, those that are compatible with allative function and the others, and so forth ; we would also, every time, set apart the verbs for which the function is a must (as the object and some locative after put) and the others for which it is just a possibility.

Establishing the connectives as a basic linguistic type formally characterized by the presence of the two connected items is an efficient barrier again an anarchic multiplication of classes.

Finally, it should be made perfectly clear that in syntax we operate only with classes, the only individual monemes that have a right to figure there being connectives, each of which being in a position to represent one of the basic syntactic units : the grammatical functions.

INVITED LECTURE

LANGUAGE AS A SEMIOLOGICAL SYSTEM: A RE-INTERPRETATION OF SAUSSURE

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0 PRELIMINARY REMARKS*

0.1 LACUS and systemic linguistics

When I first thought of coming to the Ninth LACUS Forum, I wrote to Adam Makkai to offer a slightly mischievous little fifteen minute paper in which I suggested that Chomsky, at one moment, seems to have come perilously close to becoming a network grammarian. Just think of it! What would the history of linguistics have been like if, in that solitary moment at his desk in knocking out Aspects, his eye had happened to fall on one of Michael Halliday's or Sydney Lamb's early papers - and he had realised that, as I hope to show later, their network notations in fact provide a solution to the problem he was then working on? Would he have become a stratificationalist or a systemicist - or something slightly different again? And, if he had, what would this present gathering have been like? Perhaps by now Chomsky would even have had a turn of sitting in the Presidential Chair, that is today so ably filled by Charles Hockett - Charles Hockett, whose monograph The state of the art (1968) did so much to open the eyes of the linguistics community to the less insightful aspects of Chomskyanism. So could Chomsky have become a member of LACUS and, if he had, what would be different in linguistics today? If I may borrow the sensible back-formation of a recent and linguistically innovative Secretary of State (and no doubt aspiring President), it boggles the mind.

But the result of my offer of this little paper was that Adam, beguiled perhaps by the frivolity of my topic, invited me to speak instead in the 'Invited Lecture' slot. I hope that he will not be too disappointed, in that my subject has become the apparently rather serious one of an exploration of the question 'What is the overall shape of language?' However, it still includes something about Chomsky as a

* I am grateful to the following for their comments on earlier versions of this paper: Margaret Berry, Jonathan Culler, Nigel Gotteri, Reinhard Hartmann, Dick Hudson - and others whose oral reactions I now have no record of, but which, I am sure, have influenced the present form of the paper.

potential network grammarian, and I hope that the exploration of the major topic will also provide some enjoyment - and some surprises. Indeed, I have provided, as an appendix, a summary checklist of the concepts introduced here, which you can if you wish use as a score sheet to see how well your own approach to modelling language fares in terms of what is proposed here.

I would like to thank LACUS for the honour of inviting me to give this talk. I take it, however, less as an honour to me personally than as an honour to the theory with which much of my work is associated: systemic theory. Systemic linguistics could in fact be said to have come of age this year - and in two ways. First, it is twenty-one years since the publication in Word of Michael Halliday's influential 'Categories of the theory of grammar', a work which signalled the beginnings of a new sub-school within the broad Firthian tradition. I can still remember vividly that marvellous sense of explanation that I felt, as an applied linguist working in English as a second language, when in 1964 I first discovered the important set of concepts outlined in that paper. Second, 1982 is the first year that systemic linguistics, in its manifestation as the Ninth International Systemic Workshop, has grown up enough to leave its home base in Britain. Thanks to the initiative of Jim Benson and Bill Greaves and with the support of their colleagues at Glendon College, the annual workshop takes place this year at York University in Toronto. However, I should perhaps emphasise that from their inception in 1973 the workshops have in fact always been international, and indeed intercontinental. But of more significance, perhaps, in the present context, is the fact that it is the NINTH International Systemic Workshop, just as this is the NINTH LACUS Forum. Neither grouping knew, back in 1973, that the other was forming. We were living in a peculiarly oppressive and, at times, repressive climate generated (if that is the right word) by the then dominating Chomskyan paradigm. Clearly, both sets of scholars felt the need to meet with other more or less like-minded people, in order to keep alive theories of language that were in serious danger of being completely swamped by that somewhat arbitrary set of assumptions and preoccupations (some of which have in fact some value) which John Lyons, perhaps a little misleadingly, has lumped together under the label of 'generativism'. And both groupings have survived, and indeed flourished - as also, we should note, have neo-Chomskyan formalist scholars such as Gazdar and the other phrase structure grammarians, to name but one such school. Neo-Chomskyanism lives on, and occupies a perhaps disproportionate number of positions of power. And while it is perhaps a little less subject today to the old Greek sin of hubris, so that the linguistics world today is a place of slightly greater freedom and greater tolerance, there is still some way to go. Our two organisations have, in their

different ways, played their part in this, and must continue to do so - perhaps with closer relationships in future, both with each other and with other groups such as the International Society of Functional Linguistics.

0.2 Linguistics and the Matterhorn

And so to Saussure. Or rather, first to the Matterhorn, that most magnificent of all of the mountains of the entire Alpine range, upon the summit of which I stood at nine one fresh morning about a year ago, gazing across a wilderness of snow, ice and rock to the serenity of Mont Blanc fifty miles to the West and the Austrian Alps a hundred to the East. What connection, you may ask, can there be between the Matterhorn and a paper in linguistics? Well, there are at least two, as we shall see.

The view from the summit was beyond compare, but cloud was beginning to form around us already, and soon the Matterhorn would be wearing her warning plume. In a few hours the whole mountain would be engulfed in an electric storm that would attack the mountain, cover its faces and ridges with snow, and ultimately tear away yet more massive slabs of rock and send them plunging down the almost sheer Northern and Eastern faces.

The mountain was first climbed by the Englishman Edward Whymper in 1865, and he writes movingly of the early history of climbing in the Alps in general and on the Matterhorn in particular in his delightfully titled Scrambles among the Alps (1871). There is some fascinating archive material in these pages on the variations in the dialects spoken at that time in the valleys around the high Alps, but this is not one of the connections that I intend.

Whymper followed the practice of adorning the headings to his chapters with an appropriate quotation, and for one he chose the following passage from Saussure (Whymper 1871/1900:81), in which I would draw your attention particularly to the sentence beginning 'Doubtless'. Here, then, Saussure is contemplating the Matterhorn:

What power must have been required to shatter and to sweep away the missing parts of this pyramid; for we do not see it surrounded by heaps of fragments; one sees only other peaks - themselves rooted to the ground - whose sides, equally rent, indicate an immense mass of débris, of which we do not see any trace in the neighbourhood. Doubtless this is the débris which, in the form of pebbles, boulders, and sand, fills our valleys and plains.

We all know that Saussure was Swiss, but you did not know, I suspect, that he had an interest in mountains.

Well, let me admit that I have been misleading you; this passage comes from the writings not of Ferdinand, but of Horace Bénédict de Saussure, and this Saussure was a student not of language but of physics and geology. He lived, moreover, more than a century earlier (1740-1799), and his four volumes of Voyages dans les Alpes report his thoroughgoing research in this area.*

But what, you may reasonably ask, is the relevance of this passage, which does not seem to contain any remarkable new idea? This is precisely the point. That is, while there is no idea in it that is remotely novel for us in the 1980's, the astounding fact is that in this passage of Saussure's - which is, notice, only about two hundred years old - he reveals to the reader what was at the time an amazing insight. It was perhaps the first realisation that mountains are formed (in the main) by erosion, and so the first realisation of all the geological and wider geographical 'facts', as we now take them to be, that followed from it. As Whympers says (1871/1900:83), Saussure here 'anticipated the speculations of later geologists,' and indeed the word *erode* itself is first recorded in the Oxford English Dictionary as a geological term only in 1830.

Yet the role of erosion in the formation of our physical world is something that every Western schoolchild learns and every adult takes for granted - and has done for a number of generations: Mountains, it seems - or at least their basic physical properties and the processes into which they enter - are knowable.

But is language knowable? What a contrast there is between geology and linguistics! In geology there has been the relatively rapid development of knowledge, from that first inspired insight of Horace Benedict de Saussure to, well under two hundred years later, the commonality of received facts. Yet here we are, only sixty-six years, it is true, after the publication of Ferdinand de Saussure's Cours de linguistique générale - but sixty-six years in which human knowledge has increased infinitely more rapidly than in the preceding 660 years - with still no general agreement among professional students of language on many of the most basic issues. There have been important increases in our knowledge, of course, but we still disagree on matters absolutely central to the construction of adequate models, such as 'What should the main components of a model of language be?', 'How should they be related to each other?' (which together include the question 'How many strata should there be?') and 'If there is

* The two Saussures were almost certainly related, according to André Martinet (personal communication).

a generative base in the model, where should it be?''* Perhaps one reason for our lack of agreement is, I suggest, the fact that the phenomenon that we study is not tangible, as rocks are, or indeed directly observable by any of the senses. Language is a potential or, in a computing analogy, a peculiarly rich program of options in behaving.

But surely, despite this difficulty, we can agree on at least SOME of the basic concepts that are involved, and so advance our subject a little?

0.3 Some comments on what follows

Whether or not this paper will lead to greater agreement I do not know, but I would like to use this occasion to explore with you the main concepts that seem to me to be involved in an overall model of language - these being derived more or less directly from Saussure. But, having led you up the garden path once over Saussure, I should now like to redress the balance a little by making it clear from the start that, for me at least, the train of ideas presented here leads logically towards a systemic functional grammar, broadly of the sort advocated in the writings of Halliday, Hasan, Berry, Gregory, Martin, myself and others. It is perhaps appropriate, in this year when the systemic workshop comes to North America, to remind you of some of the insights that this theory affords. I gladly recognise that other theories afford alternative and often complementary insights, and I welcome the existence of these others, both for these differences and for the many points of overlap that there are between them and systemic functional grammar - in particular I welcome stratificational and tagmemic grammar, as well as various types of 'functional' grammar. Finally, towards the end of the paper, I shall suggest as a tailpiece that one type of systemic model - my own, naturally! - has advantages over other current versions of systemic theory and, incidentally, that it may also be more in line with the ideas of Saussure. I shall certainly not expect to take everyone with me on that!

A lot of recent work in systemic theory has been focussed on discourse rather than on what we might term 'sentence grammar', and I should therefore make it clear that I shall have nothing to say here that is explicitly about discourse. My view, in a nutshell, is that there is a separate but closely related (logically but not necessarily temporally prior) 'grammar' of discourse, and that it is mediated in normal 'face encounter' circumstances by other codes as well as by language (see Fawcett 1980: Chapter 5).

* This is not to deny that there is much that linguists do, in general, agree on: see Hudson's summary 'Some issues on which linguists can agree'. (1981)

If this is so it cannot be a level of language itself, as is sometimes suggested. But it may well, as another semiotic system, have an internal organisation that has many features in common with language, so that much of what follows may in fact be relevant to discourse.

What I offer here will be a 'train of ideas', as I said just now, rather than a line of argument, because the topic is so ambitious that a tightly argued case, which would involve the introduction of detailed examples of bits of actual languages, would take several books. The normal practice, of course, when a linguist wants to argue for a new model of language (or for a modification in an old one), is to try to show that his model can handle a particular portion of a language more elegantly than others can. But in this paper I invite you to take part in a different procedure. Let us for once take for granted the nitty gritty of detailed description on which we rightly spend so much time, and let us think again about those grand, overarching ideas concerning the nature of language which are presupposed by our day-to-day work. In particular I propose to focus, as all good twentieth century structuralists should, less on ENTITIES such as the components of the model and more on the RELATIONSHIPS between them - and to go beyond mere relationships, which can be static, to what we might call 'relationships-in-process'. This is, in the terms of Winograd (1972), linguistic 'knowledge expressed as procedures'.

Next, a word of warning about the basic set of ideas to be presented here. Some of them, precisely because they are so basic, are the stuff of our introductory lectures to our first year students. Many of them are also deeply problematical, of course, and yet I can hardly go into their full complexity in the time available. If, then, I seem at times to be stating the banal, please remember that one man's axiom is another man's - or woman's - knotty problem. And I shall in any case not dwell for very long on ANY concept.

However, I do not apologise for introducing these ideas, because it is precisely these basic concepts that have, I want to suggest, been seriously 'out of true' for nearly half a century in most work in linguistics. There is a certain elegant, classical balance inherent in the proposals of Saussure, it seems to me, which has been lost. If linguistics were to adopt the set of ideas presented here - which seem to me to be no more than a logical working out of those of Saussure - this would constitute a major theoretical about-turn from the general practice - or perhaps two about-turns at once, as we shall see.

I shall draw, then, on the Bally and Sechehaye version

of Saussure's Cours de linguistique générale, as translated by Wade Baskin (1916/74). But it would not seriously affect the proposals to be explored here if you were to object that I have not interpreted his ideas correctly, or perhaps that this version of the lectures gets Saussure slightly wrong here and there. The assumption from which I start is NOT that Saussure got everything right first time, so that all we have to do is to understand him correctly. No; rather I regard him as a brilliant visionary, and his work as a superb source of insights - to only SOME of which linguists have paid due attention. This paper is not a summary of Saussure, but, as the title states, a RE-INTERPRETATION of Saussure.

We shall conclude these 'Preliminary remarks' by considering Saussure's advice as how best to discover the true nature of language. Then, in the brief Part 1 and the fuller Part 2, I shall draw on Saussure's notions, first to delimit the phenomenon to be investigated - i.e. a semiological system - and then to set up a model of its internal organisation. This will be illustrated by considering one of the simplest of all possible codes - a red and green traffic light system - and this in turn will suggest the reappraisal of at least one hallowed concept. Then in Part 3 I outline what I see as the main additional complexities of language, and so the main concepts that need to be added to those established in relation to the traffic light system. I shall suggest, moreover, that this can and should be done WITHIN that framework, essentially, rather than by building extensions on outside it. Finally (and perhaps somewhat provocatively, it may seem) I shall suggest in Part 4 that it may be both possible and desirable to construct grammars that contain not the two main levels of networks that are arguably present in a Saussurean semiotic system, but just one.

0.4 Saussure's guiding principle

We begin with Saussure's advice as to the best way of finding out what language is like. He said (1916/74:17):

If we are to discover the true nature of language,
we must learn what it has in common with all
other semiological systems.

If we want to put this advice into practice, our first step must be to establish the essential design features that must be possessed by ANY phenomenon that is to be classed as a semiological system - or, as I shall normally term it, a SEMIOTIC SYSTEM, or CODE. (These terms will be used interchangeably.) One obvious strategy for separating out the necessary from the contingent is to examine the candidate properties in relation to one of the simplest types of code, and that is why the first two parts of this paper will be

concerned as much with a traffic light system as with language.

We do not know why Saussure advocated this strategy, but one extremely practical reason for pursuing it is that whenever we begin to look in any detail at a language we tend to lose sight of the semiotic system as a whole, as we wrestle with the intricacies of a specific part of it, such as phonology, syntax or lexical semantics. The particular type of code to be considered alongside language is therefore about as simple as a code can be: it is the type of temporary traffic light system that is typically set up around a small-scale roadworks, with just a red and green light.* And to avoid the possibility of irrelevant distractions, we further assume that it has been set up on a one-way road (such as a narrow country lane that is crossed occasionally by motorway construction vehicles, the drivers of which press a control switch when they want to stop the traffic on the road).**

* As it happens, the 'units' in the traffic light code are, like those in language, discrete - i.e. they are clearly distinct from each other and do not flow into each other in a continuum relationship (e.g. as in temperature variation or the colour spectrum in nature). But such continuum (or 'cline') relationships do exist in some codes (e.g. in bee dancing, where the intensity of the dance indicates the distance of the nectar). The 'discreteness' of the values concerned is thus not a necessary 'design feature' of a semiotic system. Nor should it be thought that the inputs to and outputs from such codes will necessarily be precisely definable in the terms of some other code such as language: the code of music is a good example of a code where the 'meaning' typically defies definition in language. But this fact should not be taken as suggesting that music has no meaning: merely that the range of inputs which it encodes are rather different from those of language. The sense in which the terms 'code' and 'semiotic system' are used here is thus very wide.

** We need to add this condition in order to preclude speculation about the relationship between the messages being sent by one set of lights to road-users and those being sent by other sets whose messages are co-ordinated with them. Such a relationship is in no way, I would suggest, a syntagmatic relationship, unless one chooses to extend the scope of the term 'syntagmatic' beyond its normal sense-range, which includes, I take it, relations (1) within a code and (2) within an output from a code (see section 2.2), because each display of a light is a separate message addressed to a separate addressee. The code itself does not contain the syntactic notion of 'co-ordination'.

What, then, is a semiotic system like? It will be convenient to explore the set of concepts that seem to me to be needed by starting from five well-known Saussurian dichotomies. I shall suggest that the first two of these define the object of enquiry, and that the final three pairs of concepts, together with certain extrapolations from them, define the general properties of any semiotic system.

1 ON DELIMITING THE OBJECT OF ENQUIRY

1.1 Diachronic and synchronic studies

The first pair of Saussurian concepts is the division of linguistics (and semiotics) into the two broad areas of SYNCHRONIC and DIACHRONIC studies. For many years this was accepted as a primary dichotomy that was regarded by most scholars as both obvious and necessary. But there has been a growing emphasis in recent years, especially in the framework of sociolinguistic studies, on the desirability of introducing the historical dimension to a model of language, so that it has become necessary to justify the maintenance of the distinction. Indeed, the argument for including the historical dimension in an account of a language could no doubt be applied to the study of traffic light systems; our red/green system will have antecedents which will have influenced the shape of the present system, and which might therefore be regarded as part of any complete explanation of it.

We shall in no way be denying the validity of such studies if we nonetheless accept the Saussurian view that the first step is to make a synchronic description of a code: two or more synchronic descriptions can then be compared and related, and the dynamics of unstable elements charted. Saussure (p.80) put it like this: 'language is a system of pure values which are determined by nothing except the momentary arrangement of its terms'. And he continued: 'A value can to some extent be traced in time if we remember that it depends at each moment upon a system of co-existing values.' (We shall return later to this crucial notion of 'values'.) Thus, while diachronic descriptions are dependent on synchronic descriptions in the simplest possible sense, the reverse is not true. We may therefore attempt a synchronic description of a code, without making at the same time a diachronic study of it.

But there is a second justification for this position, which is that if we are interested in the psychological reality of the code itself as stored inside the heads of the users of that code (to anticipate the next pair of concepts), it is for practical purposes entirely reasonable to consider it simply in synchronic terms. This is because most users of

a code know hardly anything about the history of that code (and when they do know something, it is 'know' in the sense of 'know facts about' rather than in the sense of 'know the rules of' or 'know how to use'). The model developed here, then, will unashamedly accept the simplification of being a synchronic model.

1.2 From 'langue' and 'parole' to 'grammar' and 'output'

The second pair of concepts are *LANGUE* and *PAROLE*. Contrary to the emphasis given in many modern re-interpretations of these terms, *langue* and *parole* were for Saussure essentially not contradictory opposites. Rather, he suggests that one is a part of the other (pp.14-15), so that within the 'heterogeneous mass of speech facts' that constitutes 'the individual act' of *parole* we can locate the 'well-defined object' (or 'relatively well-defined', as we might wish to say) of *langue*. And he then takes the extra step of suggesting the view that 'we can dispense with the other elements of speech; indeed, the science of language is possible only if the other elements are excluded'.

A number of partially similar pairs of concepts have been proposed since Saussure, the best known and most hotly disputed being Chomsky's notions of 'competence' and 'performance'. Chomsky follows Saussure most closely in accepting the view that all 'elements of speech' but *langue* must be dispensed with - a view which depends of course on having a very clear idea as to what aspects of language are *langue* and what are not. Where he did NOT follow Saussure was in the emphasis on the 'social' nature of *langue*, in which it is seen to exist (p.14) 'by virtue of a sort of contract signed by members of a community', as opposed to the 'individual' nature of *parole*: for Chomsky the framework in both cases is individual.

Others, however, have queried the value of drawing such a sharp line between the code and the use of the code. Hymes' notion of 'communicative competence' indicates some of the limitations of Chomsky's approach (and indeed indirectly of Saussure's), and Halliday too (1973:51-4) suggests an alternative emphasis through the use of his nearest equivalent terms, 'potential' and 'actual', where each 'can be used to illuminate the other' (pp.52-3).*

* Halliday's emphasis on meaning POTENTIAL ... defined ... in terms of the CULTURE' (p.52) [my emphases] makes one wonder whether there is here an echo of Saussure, who wrote, in describing the relationship between a *langue* and its speech community (p.77): 'its SOCIAL nature is one of its inner characteristics ... but *langue* is not living - it has only POTENTIAL life [my emphases]'.

In spite of the widespread emphasis on the opposition between *langue* and *parole*, *langue* (or 'competence' or 'potential') is interpretable in all of these frameworks, including Chomsky's, as something to be included as a PART of any attempt (if such an attempt be sanctioned by the theory) to explain *parole* (or 'performance' or 'actual' linguistic events), so that in this respect they are compatible with Saussure.

But the distinction that I want to bring out here is a little different from any of these, and it is NOT a part-whole relationship. It is the one between the CODE itself, as stored in the mind of a language user as a GRAMMAR, and the OUTPUTS of that grammar, in a relationship whereby both would be parts of a model of what one might call 'idealised performance' or 'linguistic behaviour potential'. The outputs may be ACTUAL, as with a naturally occurring text, or POTENTIAL, so that a grammar can be regarded as a 'message potential'. The model of the potential may therefore be used in any attempt to explain the ACTUAL. It might be thought that the distinction that I intend is simply that between the grammar and its FINAL output (or input) - the string of phonemes (or whatever) that it turns out in ENCODING (or accepts as INPUT when used for DECODING). But I want to suggest that there is an output at EACH level, so that any text that may be produced when a code is used is describable in terms of the output at each level of that code. Thus, in the case of our traffic light system, the distinction is that between the grammar, defined as the set of rules (or program) for communicative behaviour using the red and green lights in the minds of those who use the system, whether as senders, receivers or observers of messages, and the two levels of output that we shall distinguish in section 2.1, i.e. an output such as 'go' or one such as GREEN.*

Real-life texts that get produced when a grammar is actually used are thus a special case of output, in this

* Many British linguists use the term *grammar* to refer to syntax and morphology, taken together, rather than in the sense of a 'model of language'. Saussure seems to have used the term in both senses: one instance of the sense preferred here is when he said (p.134) that 'the description of an *état de langue* is GRAMMAR' [his emphasis]. And he went on (p.136): 'although the traditional divisions of grammar may be useful in practice, they do not correspond to natural distinctions. To build a grammar we must look for a different and a higher principle.' He then went on to propose the central concepts on which we shall base our discussion in Part 2.

sense of the term, and their main value to the linguist or semiotician is as a source of data from which to attempt to infer the properties of the unobservable grammar itself. In a traffic light code, it must be admitted, the potential range of texts is boringly restricted, by comparison with a human language, but the principle holds good nonetheless.

Why have I laid such emphasis on this distinction between a grammar and its output? It is because discussions in linguistics sometimes fail to acknowledge that we need to provide descriptions of not just one or the other but of BOTH. Indeed, we need to enquire about the relevance for each of the full set of concepts to be introduced in Parts 2 and 3. For example, there is a fairly general tendency in linguistics to offer an account of the semantic structure of a given sentence or sentence-type (e.g. by some adaptation of the predicate calculus, to name one popular source of ideas), and not to go on to provide an account of the grammar of which a portion would be used in generating that sentence. We shall return to this topic in due course.*

1.3 Three requirements on an adequate model

We have now defined what it is that constitutes the essential object of study for a traffic light semiotician: it is the abstract set of 'rules' or 'generalisations' (i.e. the grammar) that specifies the potential range of communicative behaviour open to a user of this particular code, and it is to be studied synchronically. Let me now mention three requirements, as they seem to me to be, on any serious attempt to construct a model of a semiotic system.

First, I hope we may agree on a point that is axiomatic in all scientific enquiry, but which Saussure does not mention explicitly: namely, that the grammar we attempt to construct must be EXPLICIT. Thus, it will rule certain texts out as unacceptable (e.g. a blue light) and it will rule certain others in as acceptable (e.g. a red light). So far so good, but there is a complication: this is the problem of what status to give to the many insightful ideas that one encounters in linguistics that are NOT fully explicit. My view is that, where the code under investigation is particularly complex, as in the case of language, it is completely legitimate to BEGIN by constructing rather general, even vague, hunch-based models. But at some point such models must be made EXPLICIT (and thus testable, and so falsifiable) - in linguistics and semiotics as in all fields of scientific enquiry.

* In stratificational linguistics, of course, the 'output' takes the form of the activation of a given set of pathways.

A second requirement is that the model be **HOLISTIC**: that is, that it should give a reasonably complete account of the code, and neither ignore nor overemphasise any one aspect. One might take this to be a basic requirement that would automatically receive widespread acknowledgement but, as we shall see, no such assumption can be made. This requirement is not made completely explicit in Saussure's *Cours*, but it is clear, as Part 2 of this paper will show, that its spirit runs right through his approach - as for example in his advice to grammar-writers when he says (p.136): 'we should first gather together all that makes up an *état de langue* ('language state') and fit this into a theory of syntagms and a theory of associations' (for which see section 2.2).

The third requirement is that the model-builder shall have the goal of modelling **PSYCHOLOGICAL REALITY**. This is, of course, ultimately a **SOCIO-PSYCHOLOGICAL** reality, since what we are modelling is a potential for communicative behaviour. Again we are following Saussure, who emphasised (p.15) that 'linguistic signs ... are realities that have their seat in the brain', and (p.14) that '*langue* can be localised in the limited segment of the speaking circuit when an auditory image becomes associated with a concept.' (It is perhaps worth pointing out that the potential/actual interpretation of the *langue/parole* distinction seems closer to these suggestions than Chomsky's, in which it is often emphasised that the description of a language user's competence will probably be quite unlike his production and reception procedures.) We leave aside here the question as to how best to validate psycholinguistic models: this is a different matter, and we shall not discuss it here. See Fawcett (1980:9f) for two general strategies.

The question with which we began was: 'What is the overall shape of a semiotic system?', and we have now provided the first part of the answer to the question of how to **DELIMIT** such a code. (Another important aspect of delimitation will come in section 2.3.) We have therefore defined a viewpoint on what we might term its 'outward' shape. But the more fundamental questions are to do with its **INTERNAL** organisation, and it is to these that we now turn, as we consider three more Saussurian dichotomies. Part 2 will end by integrating these concepts with each other in a fully explicit model of a red and green traffic light code.

2 ON THE SHAPE OF SEMIOTIC SYSTEMS

2.1 'Signified' and 'signifier': hence 'semantics', 'form' and 'realisation'

Consider now the Saussurian concept of a SIGN (pp.66f.). Central though it is in Saussure's theory and crucial though it is here as a source of insights, I shall need to extrapolate from it in two ways, in order to bring out certain concepts that I regard as essential. Saussure's presentation of the concept, I shall suggest, is in fact somewhat misleading - and it is moreover one that goes against the overall emphasis of his theory. 'A linguistic sign', he says, 'is a two-sided psychological entity The two elements are intimately united, and each recalls the other.' The linguistic 'entity' that he then offers as an example, and which is practically always used in introductory courses, is that of the word. The familiarity of the notion of the word to the layman makes this a natural choice in an introductory context, but it leads to the misleading notion that a word is some sort of 'thing' - an 'entity', in Saussure's words. And the diagram that he uses to illustrate his point reinforces this viewpoint. (See Figure 1.)

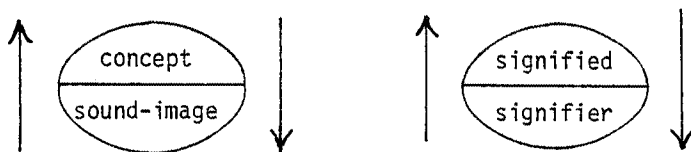


Figure 1: Saussure's representations of a word and any sign

But a word is not an entity, any more than any other sign is: it is essentially a RELATIONSHIP between, in Saussure's terms, a 'concept' and a 'sound image' - or rather, to broaden the terms so that they can be applied both to linguistic units other than the 'word' and to signs in non-linguistic codes, it is relationship between a 'signified' and a 'signifier'. It is perhaps possible that, in a discussion of such matters in a non-literate society where words had no existence as visible entities on a page, the concept of a word as an entity rather than a relationship would not have arisen. (This is not to deny the well-attested psychological reality of some word-like unit for members of non-literate societies.) The crucial point is that a sign is essentially a relationship between a signifier and a signified, and it might therefore be better

illustrated in some such way as in Figure 2.* A sentence too is a signifier that is related to a signified, but this time

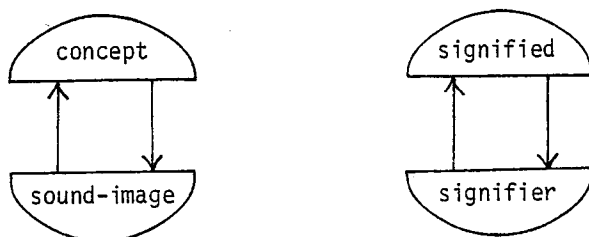


Figure 2: an alternative representation of a word and any sign

it is one that is much more complex than a single concept ('concept' being a term that we shall shortly need to examine more critically) - even in a supposedly one-word sentence. Thus, a red light facing oncoming traffic will have its own related 'signified' (which we might gloss as 'stop'). A sign is not an entity, then, but a relationship.

Indeed, Saussure's whole theory, as all the other concepts that we shall consider will bring out, is essentially a theory about RELATIONSHIPS. As he says (p.122): 'in an *état de langue* ('language-state') everything is based on relations.' And, if for a moment we broaden the perspective to include all of the sciences, it might be suggested that most and perhaps all of the crucial breakthroughs in this century exemplify the structuralist insight that it is an understanding of the various types of relationships between parts of the phenomenon under investigation, rather than a simple naming (and possibly a classification) of those parts, that leads to an adequate explanation of that phenomenon.

What type of relationship, then, is it that exists between a signifier and a signified? It is NOT that they are two complementary parts of a whole 'entity', but that the signifier expresses, or REALISES, the signified, just as the signified IS REALISED BY the signifier. The arrows in Figure 2 therefore represent this relationship of realisation. You will notice that there were in fact also arrows in Saussure's

* This characterisation of a 'word' may seem to fail to make any adequate statement about the syntagmatic relations that a word can enter into, but these can - and indeed should - be specified in a holistic model of the sort we are seeking to develop here either in the semantics or, in the case of syntax that does not directly reflect semantic relations, in the rules that relate semantics to form.

own diagram (Figure 1), and his words seem to suggest a rather more helpful characterisation of the two parts of a sign than that of two complementary parts of a whole, when he says (p.66) that 'each recalls the other.' Much later in the Cours (p.114) he introduces the term 'signification', and this seems to refer to precisely this relationship. He only does so, however, because he feels a need at that point to distinguish it from another concept (i.e. 'value', which we shall come to in section 2.3), so that he does not appear to attribute any great importance to it in its own right. Yet this relational concept of realisation (or 'signification') is, as we shall see, a crucial one in any explicit model of a semiotic system. It seems clear, therefore, that this is one of those places where Saussure did not fully perceive the centrality of the concept that he was expounding - perhaps because it was too early in the history of the discipline to be thinking in terms of constructing explicit models. Nonetheless the seeds of this concept too are clearly there in the Cours.

But there is a second and equally important extrapolation that we must make from what Saussure has to say about signs. It concerns a pair of concepts that are presupposed by a number of the other central concepts that Saussure introduces (and most particularly by that of 'linguistic value' (pp.114f.)). But nowhere does he give them the emphasis that they deserve as essential theoretical primes - as in due course Hjelmslev did.

These concepts are the two LEVELS of a semiotic system that I shall term SEMANTICS and FORM. Thus, if we regard a sign as the output from the grammar of a code, it has a signified which corresponds to one of the range of signifieds available in the semantics of the code, and a signifier which corresponds to one of the range of signifiers available at the level of form. These two levels of SEMANTICS and FORM correspond to Hjelmslev's (1943/61) two 'planes' of 'content' and 'expression', and they are the two levels that it is essential for every code to have if it is to operate at all.*

* While de Saussure does not explicitly introduce the concept of 'levels of language' as a theoretical prime, I must in fairness point out that there are parts of his discussion of the concept of 'value' (to which we come in section 2.3) which show that the general notion must have been present in his mind, if only in rather general terms. Consider, for example, this passage (p.113):

.... to consider a term [i.e. a sign] as simply the union of a certain sound with a certain concept is grossly misleading. To define it in this way would isolate the term from its system; it would mean

We shall return to a number of these concepts in later sections. For the present all we need notice is that we have worked from Saussure's notion of the sign to a recognition of the fact that every code must possess at least the two levels of semantics and form.

The acceptance of such a model has important implications for the student of language. This is because such a model shows why it is legitimate for a linguist to ignore, if he wishes, the specification of lexical items at what is traditionally seen as the THIRD level of phonology (which is what many linguists do in practice anyway). And in many - perhaps all? - codes other than human languages there is no distinction between 'form' and 'phonology' (or the equivalent '-ology'): that is, they do not have the complexity of the 'double articulation' of language. Thus, form merges with phonology in vocal codes such as whistles, with 'kinology' in kinesic codes, and with what we might call, if we wanted, 'luminology' in the case of our traffic light system. We could express the point in another way by saying that in such codes there is no distinction equivalent to that between morphemes and phonemes in language, so that, whatever the relevant level is called, it will consist in codes other than language principally of (1) 'emic' units such as kinemes, (2) the distinctive features into which they may be broken down, and in some cases (but not all, as we shall see) (3) a 'syntax' for combining them. (We leave aside here the question of the place of an equivalent of a 'systematic phonetics', in the sense of Schane 1973, and others.) We might therefore represent the internal structure of any semiotic system as in Figure 3 - while allowing for further complexity within or below (depending on one's view) the level of form. (This concept is taken up again briefly in section 3.2.)

Notice, then, that our developing model is already very different from that implied in the emphasis in mainstream linguistics since the time of Bloomfield - and on which Jakobson and other linguists who have influenced the develop-

assuming that one can start from the terms and construct the system by adding them together when, on the contrary, it is from the interdependent whole that one must start

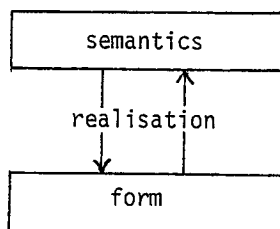


Figure 3: a first representation of the grammar of any semiotic system

ment of semiotics draw.* While expressions such as 'double articulation' and 'the duality of language' capture one of language's main 'design features', it is ironic that they tend to suggest that a model of a language should consist essentially of two levels, namely form and phonology. The use of such terms reflects the preoccupations of the 1930's and 40's, which were of course very reasonable in the climate of those times, but which are perhaps less adequate today. Yet they are to a surprisingly large extent with us still. The approach might be summarised crudely as follows: 'First get your phonology right, because that is nearest to the actual recordable substance of language. Then move on to form (morphology first and then, in due course, syntax), using the same principles as far as possible and, when something else seems to be needed to solve a problem, introducing new ones (such as the Harris-Chomsky notion of the syntactic transformation). But above all do your best to avoid bringing in 'meaning', unless you absolutely have to, because it is furthest removed from recordable substance and so least susceptible to scientific enquiry.'

There are a number of problems inherent in this as an overall research strategy for discovering the nature of language (or indeed of any code). One obvious one is that the conceptual entities and relationships that have been developed for phonology may well not be appropriate for other levels of language that are 'higher' (or 'deeper', depending

* However, one regularly finds a general advocacy of what is broadly this position, e.g. in Lyons (1977:411) who, in the course of a critique of the proposals of Katz and Fodor, remarks: 'there is an inherent connection between grammar and semantics which does not hold between grammar and phonology, and this fact should be captured in anything that purports to be a model of a language-system.'

on one's preferred metaphor). More particularly, such a research strategy will almost inevitably deny precisely that mutuality of semantics and form which finds expression in Saussure's central notion of the 'sign'. The reason for my choice of the term 'mutuality' is to express the thesis that the semantics of a code and the forms that express it are MUTUALLY DEFINING. Thus, to illustrate from the code of language, the only linguistic forms that you can use are those that carry meaning (so that *blick nog spong* is disallowed, even though the items conform to the phonological rules of English) and you cannot mean (in this specialised sense of *mean*) what you cannot say. I am therefore suggesting that the overall shape of a code will be determined as much by meaning (in a sense that we shall shortly attempt to clarify) as by form, and that while we find this mutuality in Saussure we shall not find very much of it in the work of Bloomfield and his successors. (The generative semantics wing of the transformational tradition might dispute this, but even there the contribution was more of a programme for work than holistic, in-depth description - with Chafe, perhaps, as something of an honourable exception.)

It must be said, however, that the many and various approaches to meaning that have surfaced throughout the 1960's and 1970's bear impressive witness to the fact that linguists are deeply aware of the need to find an adequate way of bringing meaning back into our models of language. I shall not try to summarise here these various approaches: most (though not all) are well known, and can be studied in what has become a considerable literature. One thing that seems now to be becoming clear is that we cannot fill the semantic gap by simply taking over the apparatus of logic. (One promising minority approach that tends to get excluded in the standard summaries is 'procedural semantics', of which Clark and Clark (1977:439f.) offer an introductory account, and we shall return to it briefly later.) But it is significant that in hardly any of this vast array of approaches to meaning do we find Saussure's emphasis on the mutuality of semantics and form. This lack may perhaps be related to another lack: namely, to the fact that in no standard textbook - not in Kempson's *Semantics* (1977) nor even in the 897 pages of Lyons' mammoth *Semantics 1 and 2*, (1977), for example - are we shown even a hint of the form that any overarching conceptual framework for handling meaning might take. All that we find are a great many bits and pieces of ideas, some of which seem to account quite neatly for one or other limited area of linguistic meaning, but which fail to link with each other in any GENERAL model of meaning. It might appear, therefore, that linguistics possesses no generally accepted framework for modelling semantics that is even remotely adequate - and even, the pessimists may say, that we should not expect to find one. (But see section 3.6 and 3.9-11.)

2.2 'Syntagmatic' and 'paradigmatic' relations

From the concept of the sign, then, we have developed the notion that any code must by definition have at least two levels; that one of these will be the SEMANTICS and the other the level of FORM and/or some '-ology'; that there will be outputs at each level; and that each will be related to each other by the relationship of REALISATION.

We come next to a fourth pair of Saussurian concepts: those of SYNTAGMATIC and PARADIGMATIC (or to use Saussure's own term, 'associative') relations. The prime importance of these, I shall suggest, is that they provide the essential concepts for the construction of precisely that overall framework for an adequate semantics that we need - but only when taken strictly in conjunction with the fifth and final pair of Saussurian concepts (section 2.3).

The introduction of this new dichotomy makes the discussion somewhat more complex, in that we shall need to ask about the relevance of these concepts to each of the two levels of the GRAMMAR of our traffic light code, and to the OUTPUTS of each of those levels - and we shall find an answer to four of these eight questions which linguists may at first find surprising. (The interrelationships of these concepts will be made rather more concrete when we come in section 2.5 to examine an explicit grammar for the traffic light code.) The essential concepts behind the terms syntagmatic and paradigmatic are too well known to need more than the briefest mention: Lyons (1977:240) offers one recent re-statement, writing: 'the syntagmatic relations which a unit contracts are those which it contracts by virtue of its combination (in a syntagm, or construction) with other units of the same level.' ('Unit' is here an atheoretical term.) And he goes on to point out (p.241) that 'although syntagmatic relations are actualised, as it were, in language-behaviour, they are nonetheless part of the language system'. Note, then, this support for the view that syntagmatic relations in language need to be modelled both in the grammar and in its outputs - something that not all theories do adequately for all levels. Lyons then goes on to give an example (from the level of form), writing (p.241): 'the lexeme "old" is syntagmatically related with the definite article "the" and the noun "man" in the expression "the old man".'

Lyons next offers what we may take as the standard view of paradigmatic relations, when he says that 'the paradigmatic relations contracted by units are those which hold between a particular unit in a given syntagm and other units which are substitutable for it in the syntagm. For example, "old" is paradigmatically related with "young", "tall", etc. in expressions like "the old man", "the young man", "the tall

man", etc.* I shall want to reconsider this somewhat form-centred definition in due course.

We come now to what may at first seem a surprising fact. This is that the concept of syntagmatic relationships is simply IRRELEVANT to many semiotic systems, and in particular that it is irrelevant to our traffic light code.** In other words, as our grammar of that code will show, it is NOT a necessary design feature of a semiotic system that it should include syntagmatic relationships, either in the semantics or at the level of form, whether in the grammar itself or in its outputs. This contrasts with the fact that it IS necessary that the grammar shows paradigmatic relations, and that the output shows in some way what has been selected in a given case. This, then, is one of the conclusions which I said earlier might come to some as a surprise.

One general question about syntagmatic relations that should be raised at this point concerns their relation to LINEARITY. I take it that there would be general agreement that in many codes other than language there are clear-cut

* It is perhaps worth noting at this point that there is one much quoted part of what Saussure has to say about syntagmatic relations where he is simply wrong: if it is an over-generalisation, it is one that is seriously misleading. This is his assertion (p.123) that 'the syntagmatic relation is *in praesentia*' ('present'): this is in contrast with the 'associative' (or paradigmatic) relation, which is, as he rightly points out, *in absentia* ('absent'). But a moment's thought will show that syntagmatic relations are only *in praesentia* when manifest in an OUTPUT from a grammar: in the grammar we find rules for their POTENTIAL complexity, so they are not in *in praesentia* any more than are the 'associative' relations. This mistake points up, once more, the need to distinguish clearly at all times between a grammar and its output.

** Where syntagmatic relations ARE relevant is in the equivalent of a 'discourse grammar', such as might be constructed to account for the relationship between one message and the next, in the simple sense that a red light follows a green, and a green follows a red. But it is not relevant either to the construction of a grammar to account for the messages themselves (which is what we are concerned with here), or to any output of that grammar, as we shall shortly see. (Nor is it relevant to the fact that in another encounter at the other end of the roadworks another set of lights may be sending synchronised messages to other addressees.)

cases of non-linear syntagms at the level of form, such as the signifier displayed on a playing card or a coat of arms. But matters become more complex if we seek to extend the simple equation of 'and' relationships and syntagms beyond this: what, for example, is the position when what we may term a 'referent' has assigned to it a number of semantic features, such as [human], [adult] and [male]? (I shall in fact suggest in section 3.5 that such cases exemplify one of a number of types of paradigmatic complexity that occur in codes that are more complex than our traffic lights system.) Saussure, however, perhaps because he was thinking at that stage in the *Cours* exclusively of language, does not seem to allow in his discussion for the concept of syntagms that are non-linear. He defines syntagms as follows (p.123): 'Combinations [of units] supported by linearity are *syntagms*, [and] the syntagm is always composed of two or more consecutive units'. However, he broadens the concept beyond linearity when he writes (p.124): 'it is not enough to consider the relation that ties together the different parts of syntagms (e.g. French *contre* ('against') and *tous* ('everyone')) in *contre tous*; ... one must also bear in mind the relation that links the whole to its parts' (e.g. *contre tous* in opposition on the one hand to *contre* and on the other to *tous*). Here, then, is an extension of the notion of the syntagm to include, besides linearity, the important notion of 'constituency' (or 'bracketing').* And in one illustrative example (pp.123-4) he compares a 'linguistic unit' to 'a fixed part of a building, e.g. a column [which] has a certain relation to the architrave that it supports; the arrangement of the two units in space suggests the syntagmatic relation.' Thus, from the perspective of semiotics as a whole, the 'linearity' of language at the level of form can be seen as a special case rather than the general rule, and it is the part-whole relationship of constituency that has wider relevance, both in semantics and form and in the grammar and its outputs.

I shall say nothing further at this point about syntagmatic relations at either the semantic, the formal or any other level. In Part 3, however, where we come to consider the characteristics that make language different from the simplest semiotic codes, we shall return to this topic.

* Strictly speaking, this is what I term 'componence' (the relationship between a 'unit' and its 'elements of structure'), which is to be distinguished from 'filling' (the relationship between an element of structure, such as subject, and the one or more units, such as nominal groups, which may fill it: where there are two or more such units they are in the relationship of 'co-ordination'). (See further Fawcett, 1980:188.)

We turn now to Saussure's contrasting concept, which is what he termed the 'associative' relation. We may interpret this, as a first approximation, as the relationship that holds between members of a set of alternative features, only one of which may be present at a time in any actual text: the relationship between the feature that is present and those that are not is thus, in Saussure terms, '*in absentia*'. (In more complex codes such as language the relationships may be more complex: see Part 3 of this paper.) But Saussure seemed to be wanting to allow for many other types of 'associative' relation when he wrote (p.125): 'mental association creates other groups besides those based on the comparing of terms that have something in common [i.e. paradigmatic relations].... the mind creates as many associative series as there are diverse relations.' The question this passage raises is therefore this: What is the relationship between the two terms 'associative' and 'paradigmatic' relations?

The answer is that, as Saussure's examples show, he uses the term *associative* in a fairly broad sense. But, by using the concept of LEVELS (which we saw earlier to be presupposed by the notion of the sign), we can account fairly satisfactorily for all of the differences between the various types of association that we find in his examples. Thus, while the French word *enseignement* is related (p.126) to *apprentissage*, *éducation*, etc. at the semantic level, its relationship to *enseigner*, *enseignons*, etc. and *changement*, *armement*, etc. is at the levels of both semantics and form (morphology, to be precise), while the relationship to *élément*, *justement* is at the level of phonology. However, it is very clear from what Saussure has to say at other points in the *Cours* - and most particularly in his discussion of the notion of 'linguistic value' - that the relationship intended is essentially that which we term today 'paradigmatic', i.e. it is the relationship that is 'based on the comparing of terms that have something in common'.

The 'something in common' between the 'terms' in a paradigmatic relationship is necessarily somewhat imprecise, because paradigmatic relations seem to occur at every level of code: at the three levels of semantics, form and phonology in language, and at the two levels of semantics and form in a traffic light code. Thus, at the level of semantics in our traffic light code we have the two contrasting meanings that we may gloss as 'stop' and 'go', and at the level of form we have the two contrasting light colours of red and green.

All this seems very obvious, and yet there is an interesting fact which suggests that to some linguists at least it may be less obvious than it appears. This is that there is in linguistics no generally used notation for representing paradigmatic relations, either in the grammar or in its outputs,

that is in any way comparable in its familiarity with the tree-diagram notation commonly used for syntagmatic relations at the level of form (and sometimes semantics). And even these are normally only used for representing OUTPUTS from a grammar. The nearest equivalent in a TG representation of an output is the attachment of features such as [+animate] to the lowest node in a tree diagram. And the notion plays a similarly low-key role in the grammar itself: the only place in which it is represented clearly in mainstream transformational linguistics is at the one rank of word, in the form of subcategorization rules (e.g. Chomsky, 1965:107). Yet the concept of paradigmatic relations is equally important in relation to higher units. In a transformational grammar, however, the few statements that there are relating to paradigmatic relations between clauses and phrases (or 'groups') are fitted in an ad hoc way into a framework that is essentially concerned with sequencing and constituency rules. That is, the phrase-structure component, which is essentially concerned with such rules, also contains curly brackets indicating alternatives and simple brackets indicating optional elements. Yet other types of paradigmatic relationship are handled, even more misleadingly, as a transformational relationship (i.e. as a type of realisation) between one sentence structure and another. It may be, I suggest, that the fundamental flaw in transformational grammar is the fact that the major preoccupation is always with syntagmatic relations - rather than maintaining what I would again term a 'mutuality' between, this time, the syntagmatic and the paradigmatic dimensions.

Notice, however, that the importance of the syntagmatic and the paradigmatic dimensions maintaining this mutuality only goes for the code of language: in most other codes syntagmatic relations play a much more subservient role, while in others again they are, as we have seen, completely absent. Thus, the only one of these two types of relationship that is obligatory in a semiotic system is the paradigmatic - and, as we shall see, it is present at each of the two levels of form and semantics. This centrality of the notion in semiotic systems as a whole may perhaps serve as a reminder that we should not underestimate the importance of this relationship for our models of language.

We might summarise our developing model of any semiotic code in the way shown in Figure 4, where the ticks indicate obligatoriness and the queries optionality.

2.3 'Substance' and 'form': the concept of 'value'

We come now to the pair of concepts that Saussure terms 'substance' and 'form'. These in fact depend in turn on the

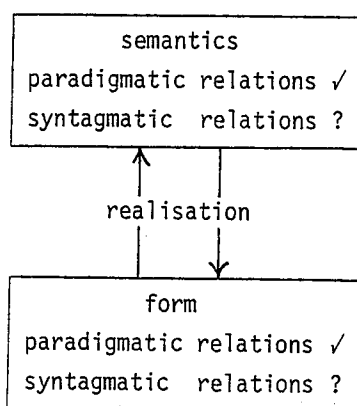


Figure 4: a second representation of the grammar of any semiotic system

yet more basic notion of 'value', and it is this notion of 'value' that we need for the construction of our model of a code. But these three terms, taken together, bring us back to the question with which we were concerned in Part 1: the delimitation of the code.

It is easiest to approach these concepts through the level of phonology, in relation to what is often termed 'phonetic substance' (in precisely the sense of Saussure's term 'substance'). 'Form', then, in this special sense of Saussure's, is the imposition of organisation on the substance: it may be said to consist, in a somewhat oversimplified approach, of the set of paradigmatically related phonemes of a language, together with the set of syntagmatic rules for combining them in syllables. It is the distinction, then, between what lies within the code and has a 'value' in that code and what lies outside it: between 'code' and 'non-code'. (Since Saussure's 'form' is our 'code', we shall in fact not need to use his term further, so that we can continue to use the term 'form' in the sense in which it was introduced in section 2.1.) In terms of our traffic light system, then, the distinction is that between those mental abstractions from the two bands of the colour spectrum that would be recognised as 'red' and as 'green', on the one hand, and all the rest of the spectrum on the other. Red and green discs of light are signifiers in the code: they are 'light-images' (to coin a term corresponding to Saussure's 'sound-image') that have psychological validity as 'emic' values in the code, as against the infinite range of possible 'etic' colours that lie outside the code.

But it is at the other end of the code that linguistics has the more urgent need for this distinction between what has

value within the code and 'substance' outside it. Saussure presents what many today would say is an oversimplified picture, when he suggests (pp.111-2) that 'thought is only a shapeless and indistinct mass', and that 'without the help of signs we would be unable to make a clearcut, consistent distinction between two ideas.' Here he is emphasising what is termed, in discussions of the design features of language, the 'discreteness' of linguistic values at the level of semantics. While there is general agreement in cognitive psychology with Saussure's view that there is an intricate interdependence between thought and language (for many types of thinking, at least), the emphasis is less upon the formlessness of 'concepts' without language, and more upon the amazing quantity of and complexity in the knowledge of the universe that we draw upon in our thinking - or 'problem-solving', to use a term that sharpens up one's appreciation of the nature of the process. The ordinary sense of the term 'concepts' that Saussure employed is far too narrow in scope to characterise this knowledge: it includes knowledge of entities, relations between entities and relations between relations; knowledge of what others know, knowledge of ways of going about solving problems, and many, many other such 'concepts', some or all of which may perhaps be stored procedurally rather than propositionally. This is what I term 'knowledge of the universe'. The problem that today faces the semanticist who is seeking to relate the forms of language to 'concepts', in this greatly enlarged sense, is principally that of where to stop.

I suggest that many linguists today are trying to include too much within the scope of semantics. In saying that, I make the assumption that we are NOT using the term 'semantics' in its extended sense, where it is a blanket label for anything whatsoever that could be interpreted as a 'meaning', but strictly as a label for a level of language (or of another code) that is tied by explicit realisation rules to a level of form - i.e. as I have earlier defined it in relation to Saussure's notion of the sign. It is not, I hasten to add, that the matters being investigated by linguists and others in this broader framework are uninteresting or unimportant: on the contrary. But the mistake is to fail to recognise that only SOME of the phenomena concerned are actually a part of the code of language.

Our red and green traffic light system illustrates this very clearly: the only meanings that it contains in its semantics are those that we have glossed as 'go' and 'stop', so that it cannot even say 'prepare to move off' (or whatever the appropriate gloss is for the meaning of a red-and-amber light). Similarly, the semantics of a language is only geared to the handling of a sub-set of the total number of 'concepts' (for want of a better word) that might be thought up at one or other

point in the history of the users of that language, and that might therefore be given value in the semantics.

This, then, is another aspect of that 'mutuality of form and semantics' which I spoke of earlier. It is here, in drawing a distinction between those 'concepts' that are a part of the code and those many other possible 'concepts' that are not, that Saussure's notion of 'value' can play a vital role.

The notion of value is in fact relevant to both syntagmatic and paradigmatic relations: let us begin with the former. 'In a syntagm,' Saussure says (p.123), 'a term acquires its value only because it stands in opposition to everything that precedes or follows it or to both.' We have already seen that Saussure's characterisation of syntagmatic relations tends to emphasise linearity at the expense of the more fundamental concept of constituency, but if we allow for this we can see that he is emphasising that a 'term' in a syntagm only has value as an ELEMENT OF STRUCTURE in a UNIT (such as the element 'subject' in a clause, at the level of form in language).

But, as we have seen, not all codes have syntagmatic relations, while ALL codes have paradigmatic relations. We have already seen how the concept is relevant to distinguishing code from non-code at the level of phonology. It is here, however, at that desperately problematical interface between what we might term 'all possible meaning' and semantics in the sense of 'meaning that is built into the organisation of a language', that the concept of paradigmatic relations between VALUES can provide a general principle for the construction of an overall semantic theory.

Let me use as a summary the following passage from Saussure (pp.116-7), in which he gives some detailed examples of the application of this concept.

Distributions of time, which are so familiar to us, are unknown in certain languages. Hebrew does not recognise even the fundamental distinctions between the past, present and future. Proto-Germanic has no special form for the future; to say that the future is expressed by the present is wrong, for the value of the present is not the same in Germanic as in languages that have a future along with the present [Other examples follow.] We find in all the foregoing examples VALUES emanating from the system. When they are said to correspond to concepts, it is understood that the concepts are purely differential and defined not by

their positive content but negatively by their relations with the other terms of the system.

Notice, finally, that Saussure gives us here the essentials of the Sapir-Whorf hypothesis, long before Sapir and Whorf, just as he gives us so many other fruitful insights.

Let us now see how the introduction of these concepts affects our developing model of any semiotic system. Essentially, the effect is to set it in a wider framework that relates 'code' to 'non-code', as Figure 5 seeks to show. Notice first the addition of 'substance', which is

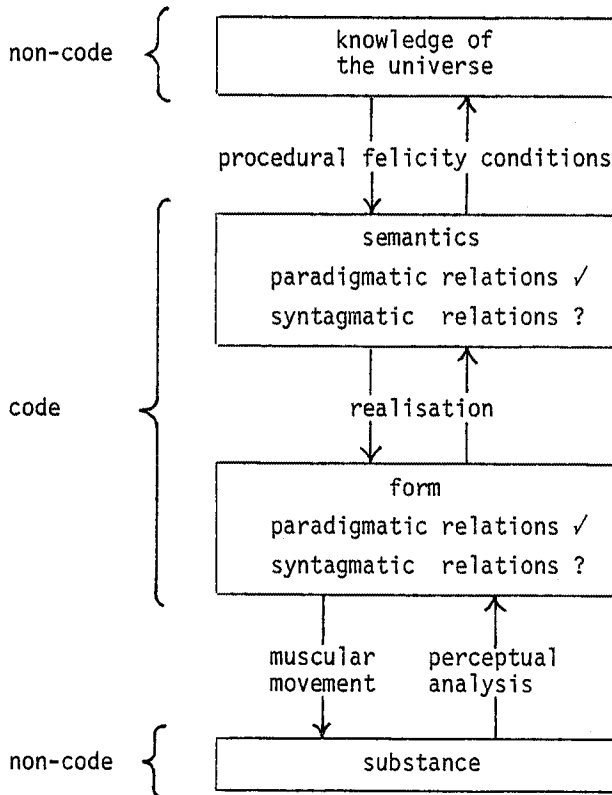


Figure 5: a third representation of the grammar of any semiotic system

the 'etic' substance of all possible sound/light/etc., and above it the words 'muscular movement' and 'perceptual analysis' which label the procedures by which form and substance are related. (Here I have used terms appropriate to

a vocal code such as language.) But more central to our present concerns is the introduction of the term 'procedural felicity conditions', as the link between semantics and knowledge of the universe. (Here the term *substance* seems peculiarly inappropriate.)*

Let me try to justify the presence in the model of this component by taking up a point where Saussure's approach has to be supplemented. This arises in the classic passage when he insists that the values in the system 'are defined negatively by their relations with the other terms of the system.' And he continues: 'their most precise characteristic is in being what the others are not' (p.117). I would readily agree that the point is valid, if our perspective is limited strictly to the level of semantics, viewed intrastratally. But the concept of 'value', in this sense, does not seem to me to yield on its own a satisfactory overall theory of meaning, and I suggest that we should additionally specify terms in a semantic system in two other ways. First, a term should be fixed, by an explicit realisation rule, to its realisation at the level of form; this is the mutuality of semantics and form that I spoke of earlier. Second, it should be fixed in relation to the conventionally agreed uses to which a semantic value (and so its formal realisation) may be put by users of the code - i.e. to aspects of knowledge of the universe.

Let me illustrate this from our traffic light code. In Saussure's 'negative definition' approach the value of 'stop' can be defined as 'not go', and the value of 'go' as 'not stop'. But now consider the meaning 'prepare to move off' that is realised in a three-light system by red-and-amber. Since 'prepare to move off' is not 'stop' it must, in a negative-definition approach, be included as part of the meaning of 'go' - with the possible consequences, if one were to act on this view in real life, of a nasty accident. But there is another possible consequence that a philosophically-minded person might think even more serious. This arises from the fact that, since 'prepare to move off' is also not 'go', it must be included in 'stop'. And to assert that it can mean the same as both 'go' and 'stop' is patently absurd.

* A semantics constructed in accordance with these principles would NOT be required to rule out sentences such as *Colourless green ideas sleep furiously*: it is the knowledge of the universe (and of possible universes) that is used in evaluating such cases, and it evaluates not the semantic stratum of the sentence but the 'referent situation' (or 'proposition') to which it refers.

Clearly, the concept of the negative definability of terms in a system is only relevant WITHIN THAT SYSTEM, and we need some means of interpreting these semantic values in terms of our knowledge of the universe. It is here, I suggest, that we may find a role for notions such as those expressed in the 'felicity conditions' developed initially by Austin (1962) for the happy performance of illocutionary acts, in the 'truth conditions' that occupied many semanticists in the 1970's, and which are really a subcategory of felicity condition, and in 'procedural semantics' (Clark and Clark, 1977:439f.; Winograd, 1972; Johnson-Laird, 1977). My term 'procedural felicity conditions' in Figure 5 is intended to capture the fact that I anticipate that a procedural interpretation of felicity conditions is likely to provide the most appropriate way of relating the values of semantic features to the knowledge of the universe, within the overall framework of the mind as a problem solver. And these procedural felicity conditions would at the same time serve as a more detailed spelling out of the labels that are put on semantic features: thus there are procedural felicity conditions for labelling a referent with the feature [human], just as there are for choosing a value such as [directive] in a very different system.*

To summarise: Saussure's notion of 'value' does not adequately characterise the total meaning of a semantic feature, and some such notion as procedural felicity conditions is required to provide a further specification, and at the same time to relate it to non-code. But these are only required for the subset of concepts that have the status of values in the semantics of the code, and what these are is determined by the availability of alternatives at the level of form, as well as by the more obvious determinant of the uses to which the code is put.

2.4 Choosing the notations for a grammar

The notations that are used to represent models on paper are important, because they affect our perception of those models. Models are probably more dependent on their associated notations than we would like to admit. As we have already noted, there is particularly widespread agreement among linguists over the notation of the tree diagram for representing SYNTAGMATIC relations in the OUTPUT from a

* Recent work in artificial intelligence by Mann and Matthiessen, which was presented at the Ninth International Systemic Workshop at York University, Toronto, 1982, incorporates 'choosers' that relate features in the system network to relevant aspects of knowledge in just the way described here and in Fawcett 1980 (where however the term 'procedural felicity condition' was not used).

language grammar at the level of FORM. (In our traffic light code, however, there are no syntagmatic relations, so we shall not need tree diagrams.) But notice that there is normally no equivalent visual representation for specifying potential syntagmatic relations in the GRAMMAR. All we find are summary descriptive formulae using abbreviated written representations of natural language (such as S, NP and VP), together with symbols such as arrows and brackets taken from the derived codes of logic and mathematics. The disadvantage of these is that they fail to show effectively the essentially RELATIONAL quality of syntagmatic relations. The widespread popularity of tree diagrams suggests that we find a better notation for relationships in diagrams using SPACE and LINES - though these have not, so far, been widely used in the grammar. (But see section 3.11, Fawcett 1980: 198-9 and stratificational grammar throughout.) This, then, is another rather surprising gap in most theories of language.

Where shall we find a suitable notation for representing PARADIGMATIC relations - and in particular paradigmatic relations in the GRAMMAR? There is one line-and-space-based notation for representing paradigmatic relations that is already in fairly widespread use and - perhaps surprisingly - there are reasons for thinking that this too can be traced back to Saussure.

We saw in the last section that he described values related paradigmatically at what we have called the level of semantics as TERMS in a SYSTEM. J.R. Firth, towards the end of his life, summarised his ideas in his 'Synopsis of linguistic theory' and, as one might expect, we find that a number of these reflect directly the views of Saussure. Thus he wrote (1957/68:186) that 'the first principle of phonological and grammatical analysis is to distinguish between STRUCTURE and SYSTEM' - i.e. between syntagmatic and paradigmatic relations. Here, then, the term 'system' is used in a narrower sense than its general sense (as when we talk of 'semiotic systems', when the term is interchangeable with 'code'). Firth then went on to describe 'the paradigmatic relations of the TERMS which commute within SYSTEMS.' [my emphases], and this seems to echo with a degree of closeness that is clearly not coincidence Saussure's discussion of the way in which 'values' are determined by their relations with the other 'terms' of the 'system'. It is therefore at least arguable that this special sense of the word 'system', can be traced back beyond Firth to Saussure. And today one of the more influential of the 'minority' theories, systemic theory, takes its name from the word 'system', understood in this sense. The main architect of the theory is Halliday, himself a pupil of Firth, and in the collection of his papers from 1956 onwards

edited by Kress (1976) one may trace a steadily increasing emphasis on this concept of system. It was in the early 1960's that Halliday developed the notation for representing a number of types of complexity in paradigmatic relations between features that we shall introduce in section 3.4. Interestingly, these concepts and their notation were taken over by Lamb, and further adapted into another of the more fully developed minority theories of today, stratificational grammar. Both theories, then, emphasise the role of relationships in the model, and both represent these in the form of 'relational networks'. What distinguishes them most clearly, perhaps, is the insistence in systemic theory that it is paradigmatic relations - and specifically choices between meanings - that lie at the heart of an explanation of language, so that these receive more emphasis than other types of relationship.

It is perhaps also worth noting that other linguists who have their background in the Firthian tradition use a rather similar notation for representing paradigmatic relations: examples are Quirk et al. (1972: e.g. 207, 508, 520), Leech (1969:104 and 1981:110, 112) and Sinclair and Coulthard (1975:31). It therefore seems appropriate to make use of this notation, if only in embryonic form, in representing paradigmatic relations in our model of the traffic light code.

2.5 An explicit grammar for a red and green traffic light code

Let us now summarise what concepts we need to have in mind in modelling even the simplest semiotic code. First, it can simply be SYNCHRONIC, and so diachronic factors can be ignored. Second, we shall distinguish between the GRAMMAR (the program of rules for semiotic behaviour using this code) and the OUTPUTS from it. Third, our model must be explicit, holistic, and it will at least aim to model the socio-psycholinguistic reality. Fourth, we shall expect to find at least the two levels of SEMANTICS and FORM, linked by a relationship of REALISATION. Fifth, at each of the two levels of semantics and form, both in the grammar and in its outputs, we may require the notions of both PARADIGMATIC and SYNTAGMATIC relations. Syntagmatic relations, however, may be absent in some codes, and in the output of a simple code paradigmatic relations may be represented by a single feature (which however implies the existence *in absentia* of the system in which it belongs). Sixth, the VALUES that are related to each other within the code by these paradigmatic, syntagmatic and realisational relationships define the boundaries of the code itself from, at one end, SUBSTANCE, and at the other, KNOWLEDGE OF THE UNIVERSE.

If you look at Figure 6 you will see that the grammar and outputs presented there exemplify all these characteristics. (Note that since it is a model of the code only, it ignores substance and knowledge of the universe.) Let us see how it works. Notice first the systemic notation used for paradigmatic relations at the levels of both semantics and form. We start in the semantics. If the entry condition [directive to traffic] is met, then one must select either the feature [go] or the feature [stop]. Notice that it is natural to provide a description of a system in the form of a procedure, since systems are inherently both RELATIONAL and PROCEDURAL. That is, a system specifies a relationship of DEPENDENCY between an entry condition and two (or more) dependent features that are in an ALTERNATIVE relationship to each other, and it can operate both as a generative/productive procedure, as in the description above, or in a classificatory/receptive mode, as when a linguist uses the model to analyse a text or in a model of language comprehension.*

We return to the choice between [go] and [stop]. (Square brackets round words in running text indicate that they are semantic features.) The only possible outputs are as shown; in an even slightly more complex code it would be impractical to list them all, and in language and other codes involving recursion it would be impossible. Let us assume that [go] is chosen: the realisation rules specify that its realisation will be GREEN, so that it is this feature in the system at the level of form that occurs as the output at that level.

Notice the double rounded brackets round the feature [directive to traffic] and around the formal feature [LIGHT]. These brackets are to indicate that these features are in fact REDUNDANT, in that they are always present in any output of the code. They are therefore non-significant, so that the code is effectively one in which there are simply

* Not that these are the same in all respects, of course. But by interpreting the *langue/parole* distinction as essentially that between 'potential' and 'actual' one can for many purposes ignore the distinctions between 'generating' and 'producing' sentences - an approach that is suggested independently by work in incorporating grammars in computer models of communicating minds.

component of the grammar		GRAMMAR	OUTPUTS
SEMANTICS	paradigmatic relations	$((\text{directive to traffic})) \begin{cases} \text{go} \\ \text{stop} \end{cases}$	$(((\text{directive to traffic})), \text{go})$ $(((\text{directive to traffic})), \text{stop})$
	syntagmatic relations	none	none
REALISATION RULES		$(([\text{directive to traffic}] \text{ r. LIGHT}))$ $[\text{go}] \text{ r. GREEN}$ $[\text{stop}] \text{ r. RED}$	
FORM	paradigmatic relations	$((\text{LIGHT})) \begin{cases} \text{GREEN} \\ \text{RED} \end{cases}$	$((\text{LIGHT})), \text{GREEN}$ $((\text{LIGHT})), \text{RED}$
	syntagmatic relations	none	none

Key: (()) = redundant feature r. = realises/is realised by

Figure 6: an explicit grammar and outputs for a red and green traffic light code

two contrasted features at each level.*

Perhaps the most obvious innovation in the grammar shown in Figure 6 (by comparison with Figures 3 through 5) is that the somewhat vague realisation relationship has grown into a separate component of the grammar. The relations that it specifies are extremely simple (as indeed

* Note that even if the redundant features were NOT redundant, the presence of two features would NOT, as one might perhaps think, have constituted a syntagm. The relationship between the two is a type of 'dependency' which, as we shall see in section 3.6, is one of a number of types of complexity that need to be recognised within the overall concept of paradigmatic relationships.

are all the relationships involved in this grammar): the 'r.' symbolises the realisation relationship and can be interpreted as 'realises' or 'is realised by', as appropriate, and the relations are one-to-one. In a simple grammar such as this we could in fact have shown the relationships even more economically by simply drawing a connecting line between each meaning and its realisation. But in the grammar of a more complex code, where the relationships are no longer simply one-to-one, there are advantages in the apparently somewhat repetitive layout we have used here. The vital point, however, is the indispensability of this component to any explicit grammar: it is indispensable for the simple reason that, if we do not state in the grammar that [go] is realised by GREEN, an automaton would not know that it should realise it as GREEN rather than RED.

To summarise: I am suggesting here that any phenomenon that is to be termed a code (or semiotic system) must consist of at least the two levels of SEMANTICS (with one class of exceptions, which I discuss briefly in the next section) and FORM, together with a REALISATION component whose function it is to make explicit the connections between the two levels. (But note that in Part 4 I shall question even this apparently incontrovertible position.) The minimal set of non-redundant paradigmatic relations at each level is a binary system of features (as in our traffic light code), and the minimal set of syntagmatic relations at each level is nil. However, I must go on to point out that some or all of the extra complexities in paradigmatic relations found in language (which will be introduced in sections 3.6 to 3.10) are present in the semantics of many codes, and that there are also many complexities in syntagmatic relations in language at the level of semantics and so of form.

2.6 An exceptional class of code: codes with no semantics

I have been using the terms 'code' and 'semiotic system' interchangeably throughout this paper. But the everyday sense of the term 'code' is rather different: it is the sense found in examples such as *morse code is faster than semaphore* and *the message was in code, so we couldn't understand it*. Such codes partly resemble those that we have been considering so far, in that they have two levels linked by a set of realisation rules, but they differ in one crucial respect. This is that while their signifiers are clearly forms (such as clusters of dots and dashes or arrangements of arm positions), their signifieds are not meanings, but letters of the alphabet.* All

* The model established here allows us to specify precisely what it is that makes a secret code secret: it is because the general public do not have access to its realisation rules. In contrast, morse and semaphore have rules of the simple one-to-one variety that are available to all.

such codes are what we might call REINTERPRETATION codes, in contrast with the PRIMARY codes that have a level of semantics. Thus the writing system of English (and of most languages) is a reinterpretation code too, in that its signifieds are the signifiers of language: roughly speaking a set of phonemes.*

However, 'ideographic' writing systems such as Chinese are quite different: such writing systems are primary codes in their own right, with a level of semantics as well as a level of form, such that the level of semantics coincides pretty well with that of the spoken language. If kinesic codes such as American and British Sign Language are to be included in the family of human languages, as it seems to me they should, so too should the Chinese writing system.**

Notice, then, that once a writing system based on phonology is brought into play, there is the possibility of a second reinterpretation: the signifieds of the writing system become the signifiers of morse, semaphore and orthography-based secret codes, so that in such cases one reinterpretation code will reinterpret the output of another reinterpretation code, which itself reinterprets a primary code. Thus one might usefully distinguish between primary and reinterpretation codes and, within the latter, between secondary and tertiary codes, and within tertiary codes, between public and private (or secret) codes. (See Fawcett forthcoming a, section 1.4, for some further criteria in classifying codes.)

Here I shall have nothing further to say about reinterpretation codes, and in Part 3 we shall restrict ourselves still further, as we consider the extra complexity of the code we call 'language'.***

* Here the realisation rules are peculiarly complex, as many studies have clearly demonstrated.

** As Charles Hockett pointed out in the discussion following the presentation of this paper at the Ninth LACUS Forum, this is additional support for my contention that the main two levels of language are semantics and form.

*** It seems to me that the notion of 'code' that is derivable from the work of Barthes (where there are two 'orders of meaning' such that the lower code may be seen as 'included', in a sense, in the level of form of the higher code) is perhaps best viewed as a metaphorical use of the concept of code as developed here.

3 ON THE SHAPE OF LANGUAGE

3.1 A language as a semiotic system

Thus far we have been drawing on Saussure (and at one point on Hjelmslev) to construct a semiotic theory, and I have been suggesting that the set of concepts presented here defines the overall shape of any semiotic system, including language. It is true that the only descriptive support comes from what may appear a ludicrously simple code. But I chose it, you will recall, as a means of ensuring the separation of the necessary from the merely contingent. Thus, if Saussure is right that in studying semiological systems in general we may discover the true nature of language, we should now be in a position to turn to that vastly more complex phenomenon.

The tendency in the past, however, has been to start from language and to extrapolate from statements about it in making statements about other codes - presumably on the somewhat dubious grounds that language is thought to be the best understood code because it is the most studied. Here, of course, we are following the alternative strategy: that we should first seek a clear picture of the design features that ALL codes have, and that then, within this overall framework, we should go on to build into it the additional complexities of language. The two purposes of this third part of the paper are thus (1) to attempt a summary of the extra complexities that are found in language - and I shall try to do this as far as I can in theory-neutral terms - and (2) to assess the extent to which these affect the basic model of any semiotic code that we have already set up.

However, this paper has already mentioned language a good many times, and I have therefore already indicated something of the way in which I see a return to the concepts of Saussure as suggesting a way forward for linguistics. Let me therefore try to summarise these at this point, if only in the somewhat general terms used so far.

I said at the start of this paper that if linguistics were to adopt the standpoint advocated here there would be one, or perhaps two, major theoretical about-turns in linguistics, and in the preceding part of this paper we have just set up the radical yet thoroughly Saussurean model that leads to this claim. The first about-turn (which for some linguists may not involve a complete about-turn) is the recognition of the central place in a model of language of a level of semantics - NOT as something added onto syntax as a supplement, and NOT as some kind of reference system relating words and syntactic structures at the level of form directly to knowledge of the universe (which is broadly the approach

which many linguists are working in today), but as a clearly constituted level of relationships between SEMANTIC VALUES, located WITHIN the code itself. And the second about-turn is directly related to the first, through this central notion of 'value'. It is the re-orientation from a preoccupation with SYNTAGMATIC relations at the level of form (and sometimes, through the use of logic-related models, at 'deeper' or 'higher' levels) to an awareness of the need to give a complementary place to PARADIGMATIC relations. It is NOT the case, I would argue, that all of the considerable syntagmatic complexity in language that has been pointed out in recent years should lead us to accept that paradigmatic relations should play the subsidiary role to which they have been assigned by many linguists. It is therefore in the conjunction of these two re-orientations - that is, in a model that emphasizes the concept of PARADIGMATIC relations at the level of SEMANTICS - together with the notion of VALUE as a means of distinguishing code from non-code - that we find the necessary framework for an overall model of the semantics of natural languages. We shall see in sections 3.6 to 3.10 a little of what this might entail.

We shall consider each component of the model at a time, and then within each we shall ask about paradigmatic and syntagmatic relations, in both the grammar itself and its outputs. Finally, we shall need to consider the question of relations between levels. But first we need to examine the notion of 'levels' in language.

3.2 The extra complexities of language: how many levels?

Figure 7, which may be compared with Figure 5, provides a guide to, and summary of, what I shall be suggesting in this third part of the paper. Note that, following the pattern set in Figure 6, the diagram shows, as well as the grammar itself, the outputs from it - with the result that the arrows linking the different components of the grammar no longer go directly from one to the other, but via those outputs.

We begin with an old question: How many levels, or strata, are there in language? In our model of a simple semiotic system in Figure 5 - and so, it might be suggested, in ANY code - there were only two levels. But when we come to language, we are confronted by its 'double articulation': the fact that 'items' at the level of form can be re-specified as smaller units in the PHONOLOGICAL component. This fact is normally expressed by positing two distinct levels of language, with semantics added as a third to yield a tri-stratal model. It is one, moreover, in which it is assumed as a matter of principle that the relationship between semantics and form is precisely the same as that between form and phonology, and it is this assumption that I want to query. It is a principle, moreover,

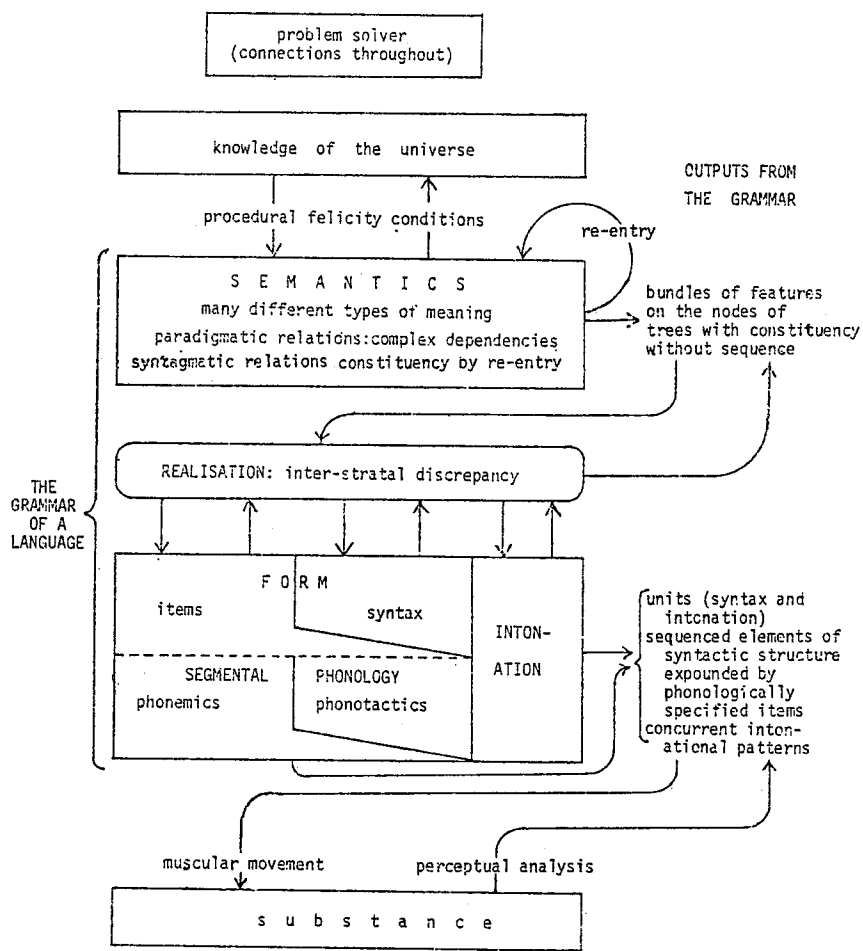


Figure 7: a representation of the grammar of English
(Compare Figure 5.)

that has been taken even further in the writings of Lamb and the other stratificationists (e.g. Lamb 1966, and Makkai and Lockwood (Eds.) 1973). These scholars have in fact suggested that a language is, as it were, a seamless web of realisation, stretching from knowledge of the universe at one end to phonetic substance at the other. At some points the number of strata is increased, so that morphology and syntax are not at the same level, but instead morphology REALISES syntax (to put it crudely, and to use traditional terms rather than the revised terminology of stratificational linguistics). For example, in Lockwood's valuable Introduction to stratificational linguistics (1972:25) there are, by implication at least, six strata (though 'phonic correlations' and 'conceptual correlations' are not there given a 'tactics'), each related to the one above and below by the same ordered and unordered 'and' and 'or' relationships.

In the writings of Halliday too there are intimations here and there of a multi-stratal model - though he frequently refers to his model as having the traditional three levels of semantics, form and phonology. At many points in his writings he describes the networks of transitivity, mood, theme, etc. as being at the level of semantics, as I would. But at other points (e.g. 1973:93) he posits a level of socio-semantic networks above these, and one might expect this to lead to a four-level model. Butler (forthcoming) goes even further, suggesting that there should be a semantics at this level that is not situation-bound, as Halliday's socio-semantic networks are, but situation-free, rather as Leech's and the many other logic-related approaches to semantics are.

However, Halliday's introduction of socio-semantic networks does not lead him to describe the new model as having four levels, because in these writings the networks of transitivity, mood and theme are seen as being INCLUDED in the level of lexico-grammar, rather than REALISED in it. The realisation relationship, which is normally regarded as an inter-stratal relationship, is now viewed as one of instantiation: it is the relationship of potential to actual or, in the terms being used here, of grammar to output. It seems to me, however, that this is to emphasise the output phase in the process of realisation, as shown in Figures 6 and 7, at the expense of the interstratal phase that is involved once one regards the networks, as Halliday clearly does, as 'deeper' (or 'higher') than mere form. So it is arguable - although I certainly would not do so - that Halliday's socio-semantic model implies a four-level model.

As an aside, I might point out that despite my difference from Halliday here, there are some remarkably close

parallels between Halliday's approach to the realisation of transitivity, mood and theme at these points and the somewhat unconventional position on levels that I shall advocate in Part 4.

All in all, then, it is likely to be an uphill task to persuade systemic linguists - let alone stratificationalists - to take seriously the view that language is, like other semiotic systems, essentially bi-stratal. But let me try! And it may be that systemic linguists, together with others who give centrality to the concept of meaning as choice, will be particularly open to the idea.

The problem centres on what we understand by REALISATION, a term taken over into systemic linguistics from Lamb. Let me begin by admitting the attraction of using the same set of concepts to handle the relationship between the different components of a model of language. But I find myself far from persuaded that all the strata proposed are strata within the single phenomenon of language. I have explained elsewhere (Fawcett, forthcoming a) why I myself find it a loss rather than a gain to draw on the same 'and' and 'or' notation to denote paradigmatic, syntagmatic and realisational relations, so I shall not repeat those arguments here. Nor shall I have time to try to substantiate all the proposals implied in the model summarised in Figure 7.

Let me simply invite you to consider the following two possibilities.

First, it may be that the relationship between language and knowledge of the universe - or any logically prior set of options in behaviour, including options in planning discourses - is rather different from the intimate realisational relationship between semantics and form - a position shared by many linguists. It may, for example, be essentially a consultative relationship, as suggested in Fawcett 1980.

Second, it may be that the relationship between form (in the sense of words and morphemes arranged in sequence) and phonemes, etc. is one in which the latter SPECIFY the internal organisation of those words and morphemes, at the level of form, rather than realising them at some lower level or levels. See Fawcett 1980:60 and 240 for a brief further discussion of this concept. Thus, as Figure 7 is intended to show, I suggest that phonemics and phontactics (or an alternative prosodic approach) is to be handled as INTERNAL to the level of form, so that a word is, as I suggested in section 2.1, not an 'entity' at the level of form intermediate between semantics and phonology, but a relationship

between features of semantics and features of phonology.

Components of an overall grammar such as phonemics and phonotactics are of course necessary, if one wishes to enquire what is and is not phonologically acceptable in a given language. But I want to suggest that it is perfectly possible to construct a grammar for use as a program for turning meanings into strings of sounds and back again without using this part of the grammar. This can be done in what I shall term a MINIMAL PROCEDURAL GRAMMAR by stating the realisation rules for certain types of meaning - i.e. meanings realised as items - directly as sounds. Note, moreover, that this is precisely what we would have to do, if we happened to be doing our linguistics in a society without a writing system, which again suggests that the level of form is ultimately phonological - just as Saussure's concept of the sign suggests.

I therefore welcome the support for this stand against the multiplication of strata in Saussure. In fact, I do not know of anywhere in his writings where he advocates the idea of a model of language with more than the two strata recognised here. (The re-interpretation codes, where the signifieds are not meanings, are clearly a quite separate type of phenomenon since, when they are employed, it is a case of two or more codes being linked together, rather than the use of a single multistratal code.)

To summarise: I am suggesting that there is not an equality among strata, each linked to the other by realisation rules, but that priority should be given to the level of semantics (or, in stratificational terms, semology) and (at this stage of the argument, but see Part 4) to the level of form that directly realises it. Meaning is intimately bound up with choice, and of the relational network approaches to language the one that lays especial emphasis on paradigmatic relationships at the level of semantics is systemic theory. It is my sense that it is these relationships that lie at the heart of language that makes me a system-icist.

Let us now look briefly at each component in turn.

3.3 Segmental phonology

In the segmental phonology component we need to model both paradigmatic and syntagmatic relations, in both the grammar and its outputs. Let us begin with the GRAMMAR. Paradigmatic relations between the distinctive features of phonemes are usually modelled in generative phonology in matrices: these are in principle simply one notation for representing fairly simple paradigmatic relations in the

grammar. (Another notation that could be used instead is that of system networks: see for example Hudson, 1974:35. We shall consider these in more detail shortly.) These phonological values are normally shown in representations of OUTPUTS as (bundles of) features. In those rare models where syntagmatic relations are represented in the output, this usually takes the form of a tree diagram of syllable structure (e.g. Fudge, 1969:253f.). Presumably such relations would be modelled in the grammar by the equivalent of a set of 'phrase-structure rules', rather as at the level of form in TG grammars, but there appears in fact to be no generally recognised formalism for expressing such syntagmatic relations in the grammar, or for expressing paradigmatic relations between such syntagms.

3.4 Intonation

A further complexity in language is the presence of INTONATION. From one viewpoint intonation may be regarded as a simple two-level code, in the sense that it consists of a set of meanings and a set of phonological realisations. But it cannot be handled as a separate code that is superimposed, as it were, on language, in the way that TONE OF VOICE ('angry', 'conciliatory' etc.) can. This is because, as even the most formally inclined linguist must recognise (e.g. Chomsky, 1970:88f.), semantics, form and intonation are inextricably interwoven with each other (e.g. in the assignment of the meanings 'given' and 'new': see for example Halliday 1970:162f., etc. whose ideas have been incorporated in Quirk et al., 1972:940f.).

3.5 Form

We turn next to the level of FORM. Here the main problem might appear to be how best to handle what is normally taken to be the very considerable degree of complexity in syntagmatic relations at this level. It is this aspect of language that has been the focus of attention since Bloomfield, through Bloch, Rulon Wells, Trager and Smith, Hockett, Harris and, above all, of course, Chomsky, whose ideas have dominated mainstream linguistics for the last twenty years. The principal new concept to be introduced in this period, in an attempt to supplement phrase-structure rules in accounting for relations between structures, has been the Harris-Chomsky notion of the syntactic transformation. Nowadays, however, the tide is flowing strongly against it - in part, no doubt, because of the failure of psycholinguistic investigations to uncover any convincing evidence for their psychological reality, in part from the difficulties they raise in a number of ways on purely formal grounds, and perhaps in part from sheer neophilia. Some prominent

transformationalists, including Chomsky himself, have concentrated on the idea of drastically reducing the number of transformations, to the point in the end where there is only one: 'Move X'. Others, such as Postal and Lakoff have been seeking to construct grammars without any transformations at all. It is the latter goal that is probably the more fashionable among the new generation of linguists - and with good reason. The transformational model as a whole must now be considered to be of doubtful value (as distinct from the many descriptive facts about English and other languages that have been discovered in the course of trying to make the model work). Thus, in terms of the goal of constructing an overall model of language, it now seems that all this effort has been misdirected, and that the excessive dominance of this theory has diverted scholars of considerable talent from pursuing more promising paths. If we start from the perspective of the overall semiotic model set up in Part 2 of this paper, we can see where things went wrong: it was that, despite the introduction of the notion of 'deep structure', transformational grammarians failed to recognise that the most important relationships between syntactic structures that have certain aspects in common are NOT what we might term 'intra-syntactic', but are to be made VIA THE LEVEL OF SEMANTICS. For example, in order to give an adequate account of the difference between the active and passive form of a clause, one needs to invoke differences of 'meaning' (and in fact differences of several different types).

However, I do not want to suggest that ALL syntactic facts have to be explained via the level of semantics. First, some syntactic relationships do not involve choice and are therefore not capable of reflecting meaning. The rule by which modifiers precede heads in English nominal groups (with a few statable exceptions) is one such rule. In systemic theory such facts are accounted for by 'structure-building rules' (the Halliday-Hudson approach) or a variant of these known as 'starting structures' (my own approach: see Fawcett 1980:46-53 for a discussion and further references). Second, there are semantically motivated types of complexity where the complexity does not directly reflect the semantics, as in the various types of what is termed 'raising' in the transformational literature, and these will be handled in the realisation component. By and large, however, syntax is complex because the 'referent situations', as I term them, that we put to each other are complex - with the degree of complexity that is possible being of course determined by what is available in the language system (as well as more general factors such as memory limitation, etc.).

Thus it is SYNTAGMATIC relations at the level of FORM (coupled with an over-simple notion of what it means for a pair of sentences to have the 'same' or 'different' meanings)

that have for too long been the focus of attention, in studies of both the grammar and its outputs. And the influence of Chomsky is such that even though his solutions have been rejected by many, his definition of the problem is still accepted. Thus it is that formal syntax is still the focus of attention for the members of the avant-garde of what many would still regard as the mainstream of linguistics, such as Heny (1979) and Gazdar (1979), to cite but two.*

PARADIGMATIC relations, however, have received little emphasis in TG (and continue to receive little in the neo-Chomskyan development that I know about). One of the very few examples of a grammar at the level of form which emphasises paradigmatic relations in syntax is Hudson's daughter dependency grammar (Hudson, 1976). However, the types of paradigmatic relations involved are the same as those in the semantics, so we shall not specify them separately in this section.

3.6 Paradigmatic relations in semantics: some relational concepts

Let us now consider the SEMANTICS, beginning with PARADIGMATIC RELATIONS in the GRAMMAR. Our traffic light code (Figure 6) possessed only a single two-term system, so that the complexity was minimal. We need to ask: In what ways are paradigmatic relations more complicated in the semantics of a human language?**

They are more complex in at least the following three ways. First, there are sets of choices at the semantic level at more than one RANK. In some approaches there are three or more; Halliday 1973:141 has three, as does the very different but still partially systemic approach of Leech 1980. I myself find that I can manage with two in what I term, borrowing a term from Halliday, the CONGRUENCE system (Fawcett 1980: 91f.), but this seems to be the minimum. Second, there is functional plurality, to which we shall return later, and, third, there is systemic complexity.

We shall begin with the third category. I shall introduce a number of concepts (which will be relevant to a little exercise that I shall shortly invite you to take part in) by showing how a simple binary system can be made more complex. The first is to add to the number of terms in a system (compare A and B in Figure 8), but this is not a major source

* I cite these particular papers simply because I have them to hand.

** One or more of these will also apply to many other codes, including a three-colour traffic light system.

of complexity: indeed, there appears to be a tendency to binary systems in natural languages. Second, a system may

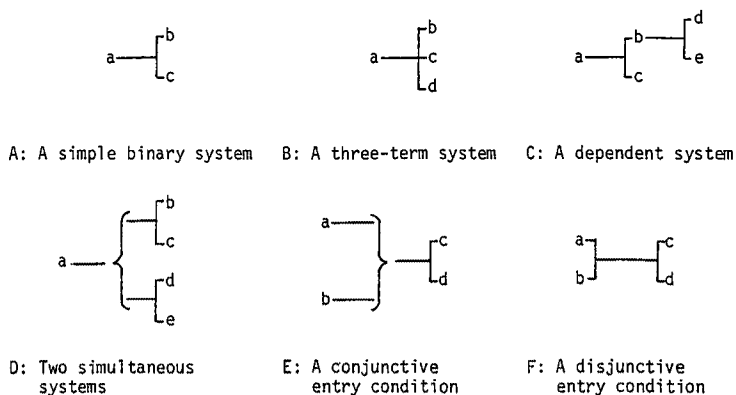


Figure 8: Some system network notations illustrating various types of paradigmatic relationship

be DEPENDENT on a term in another system. This possibility has far-reaching effects, and quite large system networks of systems largely related in a dependency relationship are a typical characteristic of the semantics of natural languages (see C in Figure 8). Third, systems may be SIMULTANEOUS; that is, two or more may be entered simultaneously (see D in Figure 8). For a stratificationalist this might seem to make the relationship a syntagmatic one, but I would argue that it is not. One reason is that such system networks can always be redrawn without the simultaneity - though with some loss of elegance - by repeating one of the systems. For example, in C in Figure 8, another system d/e could be drawn in, dependent on c, making it equivalent in generative power to D. (We come in section 3.11 to the type of genuinely syntagmatic relations that require recognition at this level.) Fourth, the entry condition to a dependent system may involve terms from two (or more) other systems: these may be in a conjunctive or a disjunctive relationship to each other, as in E and F in Figure 8. These, in fact, are the systemic relationships developed by Halliday in the early 1960's that we spoke of in section 2.4, and they are set out in a number of places, e.g. Halliday, (1967:38 and 1973:47), Berry (1975:177f.), Kress (1976:14), Fawcett (1980:20).

3.7 Interlude: setting the problem

These concepts may seem at first sight relatively unfamiliar to anyone who has been habituated to focussing on the set of syntagmatically-oriented notions considered central in mainstream linguistics, and in any case not of great importance. However, by a brief analysis of part of Chomsky's Aspects of the theory of syntax (1965), I shall now show (1) that many of the concepts ARE in fact in use in transformational theory, though in a different guise and only in a limited field of application, and (2) that Chomsky himself attached importance to them - and to the question of the notation used to represent them.

The pages in question are those where he introduces the notion of 'subcategorisation rules' (1965:esp. 82f.). These are simply a set of classification rules, and I shall use that simpler term here (as does Hudson, 1976:27f. and 182f., who also discusses the relationship between such rules and system networks). First Chomsky outlines (p.82, example (20)) a set of classification rules for nouns in English and, despite the fact that he has previously (p.79) rightly emphasised that these are 'subcategorisation' rather than syntactic 'branching' rules, he uses the 're-write rule' notation familiar from syntactic phrase structure rules. These begin (p.82) as follows:

- (20) (i) $N \rightarrow [+N, \pm \text{Common}]$
 (ii) $[+ \text{Common}] \rightarrow [\pm \text{Count}]$ etc.

There is no need to reproduce the rules in full because, as he goes on to say (p.83): 'the total effect of the rules can be represented by the branching diagram (23)', and this is reproduced below in Figure 9. Why, we might ask, does he offer this alternative notation at all? The answer can only be that he considers it a valuable means of communicating to the reader the relational concepts that are involved. There are a number of good reasons for preferring a network notation of this sort; one is the advantage of lines and spaces over a verbal description for representing abstract relations that I mentioned earlier, and another is the simple way in which one can follow a pathway right through the network, collecting a bundle of features on the way. This makes the rules more economical, because it avoids the wasteful procedure of reading each of the left-most features of the rules lower down the page in a search for the next relevant rule, the first part of which is simply a redundant copy of the feature that one has just collected.

Chomsky's diagram is in fact a system network: that is, it is an attempt to represent the relationships that hold between certain of the features of English nouns by setting

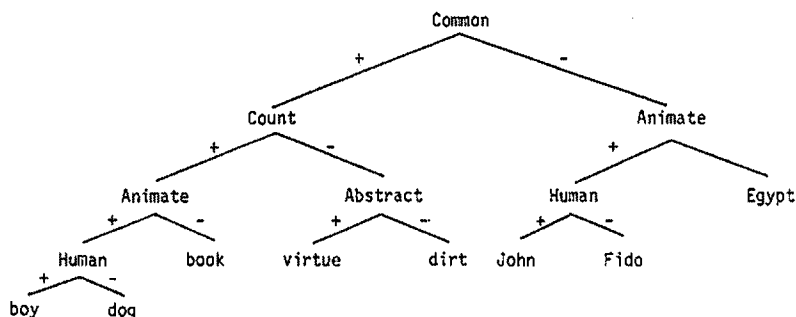


Figure 9: Chomsky's classification for nouns in English

them out in a notation that communicates those relationships to us through the use of lines and space. I shall here make the assumption that labels such as [+animate] do not in fact refer to syntactic features, however 'deep' as Chomsky suggests in *Aspects*, but to SEMANTIC features: in other words, I take it that there is nowadays fairly widespread agreement that what Chomsky was attempting here was an exercise in semantics: compare Leech (1974:121). However, the point that I am making is not weakened if you do not accept this view: the discussion should merely be transferred to the preceding section on form.

You will notice that Chomsky sees each system as the presence or absence of some feature: this works well in many cases, but in some binary systems it forces one to give the appearance (if no more) of unmarked status to one of two equally weighted candidates: for example, should we prefer [+Abstract] or [-Concrete]? And, in those cases where there are three or more features in a system, the notation breaks down completely (unless one introduces a redundant feature to restore binariness, which is at the very least a dubious tactic). In Figure 10 I therefore offer an alternative version of Chomsky's diagram that conforms to the usual systemic layout: it has been turned through ninety degrees and it labels the features between which one chooses, rather than labelling the entry line and choosing between [+] and [-]. (The labels themselves have not been altered, in order to facilitate comparisons, though some could be

changed with advantage, as implied above - e.g. [- count] to [mass] and [- abstract] to [concrete].

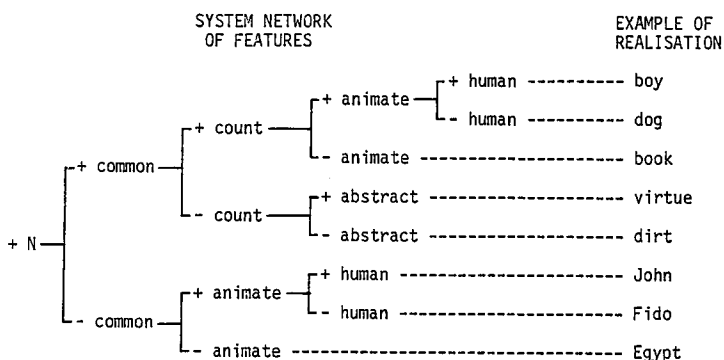


Figure 10: An alternative representation of Figure 9

However, most systemic linguists would want to re-work Figure 10, because it repeats a number of systems. (You may also be dissatisfied with it on purely descriptive grounds, but it will not affect the point at issue if we accept it for present purposes as an adequate account of the relevant features.) Interestingly, Chomsky too was dissatisfied with his own representation of the rules, whether expressed in the form of re-write rules or as a network. He therefore next offered what looks like a more elegant version of his original set of rules, in which six rules have been reduced to four. It runs as in Figure 11.

- (22) (i) $N \rightarrow [+N, \pm \text{Animate}, \pm \text{Common}]$
 (ii) $[+ \text{Common}] \rightarrow [\pm \text{Count}]$
 (iii) $[- \text{Count}] \rightarrow \begin{bmatrix} \pm \text{Abstract} \\ - \text{Animate} \end{bmatrix}$
 (iv) $[+ \text{Animate}] \rightarrow [\pm \text{Human}]$

Figure 11: Chomsky's revised subcategorisation rules for nouns

We come now to the first point of particular interest: Chomsky suggests (p.83) that 'in this case there is no representing branching diagram'. And he goes on: 'if we were to require representability in a branching diagram as a formal condition on these rules, then (22) would be excluded.'

Here, then Chomsky seems to be acknowledging the strength of our need for a less language-like code for representing the code of language - while being at the same time forced, because he believes that 'there is no representing branch diagram', into using his usual format of quasi-mathematical re-write rules (mathematics being itself a code closely related to and ultimately derived from natural language).

However, Chomsky's assumption that there is no equivalent 'representing branching diagram' is unfounded (unless one adds the requirement that branching shall be unidirectional). But before we ask what such a diagram would be like, I must first point out a surprising and perhaps significant fact. This is that it is by no means easy to interpret just what Chomsky's rules are saying. Notice first that rules (i) and (ii) allow us to generate the bundle [+N, +Animate, +Common, -Count]. (The further choice in rule (iv) between [+Human] and [-Human], to which our choice of [+Animate] leads, is irrelevant to the present argument.) Yet Chomsky surely does NOT want to provide for this array of features, since the list of nouns whose features he is specifying does not include any that are both [+Animate] and [-Count]. Presumably his rule (iii) is intended to solve this difficulty, and it tries to do so by making use of a convention that is in fact not explained; i.e. that the inclusion of a single feature such as [-Animate] on the right of a rule is to be interpreted as a CONDITION on the choice above it, such as that between [+Abstract] and [-Abstract]. (The interpretation that an unsuspecting automaton might make could be that such a single feature must be included in the bundle if the leftmost feature of the rule, [-Count], is chosen.) But the acceptance of such a convention is still insufficient to get the required result, and we must FURTHER say that we will not allow ourselves to choose a feature (such as [-Count] in rule (ii)) if one cannot then go on to choose between the dependent features. It is only if we add all these inelegant extra requirements to the interpretation of Chomsky's sub-categorisation rules that something may be saved from the wreckage of what at first sight looks like a neat set of rules. Yet Chomsky appears to see here no difficulty in the model or its associated notation.

If, however, we switch to the more open display of a system network notation, we may find a solution to the problem without too much difficulty.

Let me invite you at this point to take up pencil and paper and to try to turn the network shown in Figure 10 into one in which no feature occurs more than once. To do this you will need to employ some at least of the concepts intro-

duced in Figure 8 - and which Chomsky, despite his mathematical background, seems not to have been aware of.

3.8 Interlude: the solution

The problem is in fact simply that of how to provide for the fact that the particular categories of entity for which we use proper names (i.e. those nouns that are [-Common]) are the same as those entered after selecting [+Common] and [+Count]. The network display could, of course, be translated into a set of re-write classification rules, if that were thought to be advantageous, but this would be less economical, as we have already seen, as well as having the effect of suggesting a misleading parallel with the very different concept of rules for syntactic constituency, which are also expressed in such re-write rules. The solution to the problem is given at the end of the paper.

The fascinating question with which this study of Chomsky's unrewarding excursion into network representations leaves us is this: What direction would his linguistics have taken if he had been aware that there already existed a fully adequate 'branching notation' for these relationships? Would it have led him to an increased awareness of the importance of paradigmatic relationships? And would it also have led him, in relation to his network, as it led Halliday in relation to his networks, to the view that the features in it are options in MEANING, and so may be regarded as SEMANTIC system networks? The answer must be: Perhaps, but not necessarily. Hudson, for example, is one systemic linguist who took the first of these two steps but not the second; his networks, which were initially very likely Halliday's, became during the 1970's more and more explicitly tied to the level of form. (Indeed, the relationship between them and their output should not, strictly speaking, be termed 'realisation', since this is normally used in linguistics to refer to an inter-stratal relationship.) For Chomsky to have followed a Hallidayan route would have required a number of other changes - principally, the rejection of his 'interpretive semantics' approach, in which the semantic component is merely interpretive of the 'deep structure' component, and the adoption of the 'generative semantics' position, in which the semantic component is seen as the generative base of the grammar.* And he would have

* I do not accept Chomsky's view that the two are merely notational variants of each other: the implications for the overall model of language are too serious for this.

had to introduce system networks to many areas of the grammar other than the 'sense' of nouns, these other areas being definable in two ways, as we shall see shortly. But any or all of these developments could have followed, once he had discovered the heuristic and descriptive power and economy of complex branching notations - i.e. system networks - as a means of exploring and summarising the nature of paradigmatic relations in language. And, if he had made this discovery and announced it in Aspects, the history of linguistics from 1965 on might perhaps have been rather different.

3.9 Paradigmatic relations in semantics: Lyons and Leech

It would be unfair, however, to suggest that Katz, Fodor, Chomsky, and others working in the transformational tradition, such as the generative semanticists, were the only ones to show concern about the anomaly of the lack of an adequate semantics in the models of the 1950's and 1960's. Lyons, for example (1963, 1968, 1977) introduces paradigmatic relations to his discussions of semantics - at least, to the limited area of lexical semantics. He provides (e.g. 1977:720f.) a particularly thorough treatment of the various types of conceptually relevant paradigmatic relationship (gradable and non-gradable contrasts, binary and non-binary contrasts, converseness, etc.). Yet in bringing out these differences there is a danger of obscuring the central fact that, in terms of the LANGUAGE SYSTEM ITSELF (as opposed to what we may formulate for ourselves about what types of relationship are conceptually possible) all the words considered are simply realisations of, in the words of Saussure, 'values emanating from the system', and so of paradigmatically related features at the level of semantics.

A discussion of the same area of language which takes a position very much closer to that advocated here is that of Leech (1969, 1980). His proposals are noteworthy, because he relates semantic features of the types discussed by Lyons to each other in network diagrams showing dependancy and other more complex paradigmatic relationships. But at the same time he adds a notation for the TYPES of contrastiveness on which Lyons concentrates - something which a systemic linguist might capture instead in the wording of the entry-condition to the system-network or in a name for the network. But clearly for Leech it is the contrasts that matter, and in essence his diagrams are system networks of precisely the sort that we have been discussing in relation to Chomsky's proposals in Aspects: see for example the diagram in Leech 1980:112.

But how adequate are these various approaches to semantics? The little network that we have been examining

covers only a small part of the semantics (and that quite inadequately, in fact) because it deals merely with certain sense relations of entities that are realised as nouns. There are in fact two main dimensions beyond this that must be taken account of in a holistic model of the types of paradigmatic relations that we find at the level of semantics.

The first is the type of rank relationship mentioned in section 3.6, and the second is the need to indicate the range of types of meaning other than 'cognitive' or 'experiential' meaning that are to be included. As Lyons says (1977:415), in criticising the approach to semantics of Katz and Fodor (1964) (with which Chomsky's proposals in Aspects were of course very closely linked):

No account was taken of the fact that the semantic relationship between a declarative sentence and an interrogative sentence, or between a declarative sentence and an imperative sentence, has a different kind of semantic relationship than that which holds, or may hold, between two declarative sentences It is now widely recognised that there are different kinds of meaning to be accounted for in the analysis of language systems.

One of the virtues of Lyons' two-volume book Semantics is that it gives particularly generous treatment to types of meaning other than the 'cognitive' or 'experiential', such as thematic meaning, deixis, illocutionary force, negation and modality. But he does not even discuss possible ways in which these might be integrated within an overall semantic theory; and for this we must look elsewhere.

3.10 Paradigmatic relations in semantics: the systemic functional approach

One of the strongest - and so most interesting - claims as to how all of the 'different kinds of meaning' may be related to each other in a grammar is that of Halliday, through his notion of FUNCTIONAL COMPONENTS. This claim forms the focal point of many of his writings in the early 1970's, and essentially it is as follows (taken from Halliday, 1970:142):

When we examine the meaning potential of language itself, we find that the vast numbers of options embodied in it combine into a very few relatively independent "networks"; and these networks of options correspond to certain basic functions of language.

Halliday has proposed that there may be three and perhaps four of these (e.g. 1970:143, 1973:141, 1977:132), while I

have suggested the value of distinguishing eight clusters of networks in the core semantics, each corresponding to a clearly distinguishable type of meaning. Here I can do no more than list the names of the main components that I would recognise. They are: experiential, logical relationships, negativity, interactional, affective, modality, thematic and informational. For further exemplification and arguments for recognising these eight, see Fawcett 1980:28f. For many linguists and applied linguists, it is the model's ability to distinguish clearly these various types of meaning and at the same time to relate them to each other that gives systemic theory its particular appeal; hence the name that is being increasingly used of 'systemic functional grammar'.

So far we have been considering paradigmatic relations in the grammar. The representation of semantic features in an OUTPUT causes no great problems except one of quantity; they can be shown at each appropriate node of a structure. But, given the complexity of the semantics, there may be many at each node.

3.11 Syntagmatic relations in semantics

Till this point we have mainly been concerned with paradigmatic relationships, but we also have to recognise the presence of SYNTAGMATIC complexity in the semantics of natural languages. We saw in section 2.2 that Lyons (1977) related the notion of paradigmatic relations to (a place in) a syntagm. He gave examples from the level of FORM, and also from graphology (which he used, I take it, as an equivalent to phonology that would not raise problems for the non-linguist reader). But he gives no examples from the level of semantics, at this point in the book.

The problem of representing the semantic structure of the OUTPUT from a grammar is approached in a number of works, including Lyons 1977, and these usually depend more or less explicitly on notions drawn from logic. One early contribution was that of Leech (1969, 1974/80), and it was significant as the first real attempt to present proposals that integrated syntagmatic and paradigmatic relations at this level.

But even here, surprisingly, there are no suggestions as to how syntagmatic semantic relations will be handled in the GRAMMAR: he only deals with the syntagmatic relations of OUTPUTS from the grammar (essentially in the terms, familiar from logic, of predications and their arguments).

What, we might ask, has Halliday to say about the notion of syntagmatic relations at the level of semantics? The answer must be: Disappointingly little. Even in his most

semantically oriented writings we find no suggestion of the form that semantic structure will take, either in the grammar or in its outputs. Yet in both there must be at least (1) the sister-sister and sister-mother relationships of constituency, and (2) the relation of co-ordination (even though the notion of linearity may not be involved in either case). However, specifically systemic proposals regarding syntagmatic semantic relations, in both the grammar and in its outputs, can be found in Fawcett 1980:197f. Perhaps the most interesting point is that semantic syntagmatic relations in the grammar are shown by RE-ENTRY lines.

To summarise sections 3.6 to 3.10: I find in systemic functional grammar the richest and most rewarding approach to the problem of modelling the immense complexity in paradigmatic relations between semantic values. And I have been able to develop a concept and notation (found in earlier systemic writings as a means of handling recursion) into a means of handling the much broader concept of semantic syntagmatic relations in the grammar.

3.12 Inter-stratal relationships

Here I shall not say anything about the question of the relationship between form and phonology, since, as I suggested earlier in section 3.2, I regard it as one of 'specification' rather than realisation. That is, segmental phonology 'specifies' the phonological shape of a 'word' WITHIN - rather than inter-stratally 'realising' - the level of form.

Here I am concerned with complexity in the way in which the grammar of a language (or another code) should handle relations between semantics and form: this is the REALISATION component. Clearly, in language these relationships are very often NOT of the one-to-one variety illustrated in our traffic light code. We frequently find that the realisation of a semantic feature in a particular form depends on the selection of a particular term in another system. As a simple example consider the meaning 'mouse' (which we shall characterise here by the Latin term *mus*). We may say, then, that 'mus' is realised as *mouse* if [singular] is chosen in the NUMBER system and as *mice* if [plural] is chosen. Various notations are available that can express these facts, including line and space representations such as those used by stratificationists, but there are good reasons for preferring some such column notation as that shown in Figure 12, one being that we sometimes need to state NEGATIVE conditions on such realisations, which this notation does economically (e.g. Fawcett 1980:118). The example given is relatively simple, but inter-stratal discrepancy is often rather more complex than this, and it occurs with higher units as much as

FEATURE	CONDITIONAL FEATURE	REALISATION
mus	singular	h ◁ mouse
	plural	h ◁ mice

Key: h = head (in a nominal group)

◁ = is expounded by

Figure 12: A notation for inter-stratal realisation

it does with words. Indeed, the notion of the syntagmatic transformation was introduced into TG grammars for precisely the purpose of showing a way of relating two 'surface' strings (such as the active and passive forms of a clause) to what was thought to be a single underlying 'deep structure' (and so ultimately to a single semantic interpretation). If neo-Chomskyan linguists really abandon transformations completely (and it is arguable that Gazdar, for example, has merely shifted them to a still crucial meta-grammar), it will be necessary to introduce some other means of handling those frequent cases where there is not a one-to-one relationship between semantics and syntax. Indeed, I suspect that there is no other means of doing this than by having realisation rules that include conditions, essentially as in Figure 12 (though many different notational variants for such rules may of course be suggested).

3.13 The extra complexities of language: summary

In this third part of the paper we have introduced a large number of types of complexity that were not found in our traffic light code. It might be suggested that they are such that they will seriously alter the picture of a code given there, but I do not in fact think that this is the case. The reason is, in essence, that we have in every case not **ALTERED** the basic concepts but **ADDED** to them - as Figure 7 shows.

Perhaps the main additions are these: (1) the introduction of syntagmatic relations to both semantics and form; (2) the immense explosion in paradigmatic complexity in the semantics, in terms of (a) rank, (b) systemic complexity and (c) functional components; (3) the recognition that the realisation rules will not be of the simple, one-to-one type; (4) the specification of items at the level of form in segmental phonology (phonemics and phonotactics); and (5) the addition of intonation. All these are shown in Figure 7, which can be compared with Figure 5. The comparison brings out the essential similarity between a language and any

simple code.

I would claim, in fact, (1) that most, and perhaps all, codes can be located somewhere on a multidimensional scale, the two end-points of which may be represented by the binary traffic light code and language, and (2) that any such classification must be made in terms of EACH of the broad dimensions of variation that we have introduced. In other words, I am suggesting here a basis for a typology of semiotic systems which complements that mentioned in section 2.6.

Let me finally emphasise that nothing that we have added has altered the fact that the basic shape of the code of language conforms to that of ALL codes: essentially there are the two levels of semantics and form, and a set of realisation rules that relate them. And, at each end of the code - where it relates out to knowledge of the universe and to substance - the paradigmatically related values that are incorporated in the code are simply a subset of all those values that MIGHT have been incorporated in the code, but are not. It is therefore crucial for the linguist to find a way of determining which values are a part of the code and which are not, and it is here that the notion of the mutuality of semantics and form - and so the realisation rules that express this mutuality, play the crucial role in determining which meanings have a value in the system and which have not.

4 TAILPIECE: AN ISSUE FOR MINIMAL PROCEDURAL GRAMMARS

4.1 How many levels of networks?

Before concluding, let me share with you an issue concerning levels currently under discussion within systemic theory. The problem is relevant, however, to all theories.

As we saw in section 3.2, one view found in systemic theory is that we may happily add further levels to the model as necessary, rather as stratificationists have in the past. A second viewpoint follows the traditional tri-stratal model of semantics, form and phonology - and there is a traditional streak in most systemicists. It may be, perhaps, that my fellow systemicists will not object too much to my proposal that segmental phonology is not a realisation of form but an internal specification of items at that same level; I do not know, because although the idea appears in Fawcett 1980 I have never given it great emphasis and I know of no published discussion of the suggestion. It is essentially a programmatic proposal whose status can be no more than that until it has been subjected to the informal test of incorporation in some piece of extended descriptive work.

I have made a second proposal which might be regarded as the abolition of a level - but which is really a 'setting aside', as it were, of a major aspect of it, i.e. of paradigmatic relations. This proposal certainly has caused some controversy, so let me sketch it out for you.

4.2 System networks in the traffic light code

It will be simplest to consider it in relation to the grammar for our traffic light code shown in Figure 6. As you may have noticed, it is 'obese' - to borrow Davey's (1978) term for a grammar which is considered to be excessively uneconomical.* That is, it can operate perfectly satisfactorily if one of what we have assumed to be its essential components is removed. Which is it? We have seen that the realisation component is necessary, and our simple code has no syntagmatic relations, so it must be paradigmatic relations at the level of either form or semantics. Which is more essential? One route to the answer is to remind ourselves that codes are by definition for use in communication; the purpose of a code is to transmit not mere forms, but meanings. It is meanings, then, that are at the outer end of the communicational process between two minds, such that the chain is: meaning - form - substance - form - meaning. The obligatory component is therefore the semantics, and it is thus the component in which formal paradigmatic relations are modelled that is at risk.

Let us therefore remove it from Figure 6 and quickly check whether the grammar will still work without it. First, the features [directive to traffic] and, let us say, [go] are selected in the semantics. The first of these is, as we have seen, redundant, so we can ignore it. The output is thus effectively [go]. This output acts as the

*Halliday, however, regularly asserts, as he did at the Ninth LACUS Forum, that he likes grammars that are 'extra-vagant' - here displaying his well-known rhetorical skills. Notice that this quasi-antonym of *economical* connotes not approval, as one might expect, but a mixture of establishment disapproval and anti-establishment approval that both shocks and delights, so appealing strongly to the anti-establishmentarianism in most LACUS members! But extra-vagance/obesity in grammars can easily be overdone, and the degree of economy found in Halliday's own descriptive work suggests that what is intended is really a warning against the narrow-minded pursuit of economy at the expense of other and more important matters. Let me suggest that grammars (and particularly, perhaps, those that give a central place to what we may mean) should also normally strive to be mean.

input to the realisation rules, rather as shown for language in Figure 7, and the realisation rule for [go] tells us that it will be realised as GREEN. Notice, then, that we have not needed to take this feature and re-process it through the paradigmatic relations component at the level of form: we can move directly from the realisation rules diagonally to the right, as it were, to the output at the level of form, where one of the two possible outputs is GREEN. In other words, we do not seem to need paradigmatic relations at the level of form in Figure 6. (Note that this is a different matter from saying that we do not need syntagmatic relations in the grammar at that level, in codes that have syntax, as we shall see shortly.)

So far we have been considering the generation/product-ion of forms. What about analysis/reception? The same principle applies, and we can move straight from instances of a red traffic light, through the realisation rules, and to the meaning 'stop' in contrast with 'go'. I do not deny, of course, that there are CONTRASTS at levels other than semantics - how could one? - but I do suggest that the notion that a red light is in contrast with a green light is not in itself very significant, and furthermore that it is also plausible in psychological terms to see the contrast as operating VIA THE SEMANTICS. In other words, it is because there are two meanings 'go' and 'stop' - the distinction between them being rather important, since it is potentially a matter of life and death - that we distinguish so clearly between red and green traffic lights.

4.3 Exhaustive and minimal procedural grammars

How can I possibly deny the importance of the contrast between red and green? every semiotician will ask. The answer - and this is perhaps the key to the misunderstanding that may occur - is that there is a crucial distinction between (1) the facts which a semiotician or a linguist needs to know about a code in order to state its properties exhaustively (including a language's rules for syllable structure), which I shall call an EXHAUSTIVE GRAMMAR, and (2) the subset of those facts which she or he must incorporate in an explicit MINIMAL PROCEDURAL GRAMMAR, i.e. one that could be incorporated in an automaton and used to turn meanings into sounds or letters, and sounds or letters into meanings. It would be perfectly possible for a procedural grammar to run everything through all the components that would be needed in an exhaustive grammar - as I think stratificational grammars as presently conceived would do - and this is why I need to specify that such a grammar would be a maximally economical one. It is the construction of such minimal procedural grammars that I take to be one of the main challenges to linguistics today - most

particularly in the computing mode.

4.4 A minimal procedural grammar of language

What goes for the traffic light code goes for language. It does not matter, for example, that the bi-uniqueness of the traffic light code's realisation rules is replaced by the inter-stratal discrepancy of language.

I should emphasise, however, that it is paradigmatic relations that we are considering, and not syntagmatic relations, and that the grammar I am currently working with, which is in the terms suggested above a minimal procedural grammar, does indeed have syntagmatic relations at the level of form. These are termed 'starting structures', and they specify potential sequencing relations among the elements of structure of units such as the clause and nominal group. These have the important effect of making the realisation rules very much simpler. Thus, there is something - though not very much - in the grammar at the level of form. On the other hand, the contrasts between the alternative possible structures - which one could, if one wished, model in system networks - are needed only in an exhaustive grammar (such as the sort that Butler is working on, in the systemic framework).

This is not the place to go further; a fairly rich grammar for some central portions of English constructed on these principles is set out in Fawcett 1980.

Surprisingly, even this apparently unconventional advocacy of a model that works with only a single stratum of system networks finds a hint of support in Saussure - who, in introducing his central concept of the sign, says of the written form *arbor* that it is called a sign (in layman usage, he means, as opposed to his special sense of the term) 'only because it carries the concept "tree"'. In other words, for a moment Saussure himself seems to be elevating the semantic value of the signified above the formal value of the signifier.

5 CLOSING REMARKS

5.1 Conclusions

The reader will have noticed that I found it desirable to introduce certain terms from systemic theory in order to describe some of the types of complexity introduced here. I hope this will not have caused difficulty: wherever theory-neutral terms have been available I have tried to use them. The essential point is that these types of complexity exist,

whatever terms are used to describe them, and any model of language that seeks to be holistic must find a way of handling them.

But it is no accident that I have found myself writing in later sections increasingly in systemic terms: this is because it is this model that gives the most central place to those aspects of language to which our attention has inevitably been directed by our attempt to return to the basic concepts of Saussure.

Most, and perhaps all, of these concepts could also be expressed in terms of a stratificational grammar, and the two theories have much in common - more, perhaps than many practitioners in each realise. One of the differences, however, is that at present we do not find in stratificational grammar systemic theory's particular emphasis on paradigmatic relations at the level of semantics as the basis of the organisation of language. But there is no reason of principle or practice why this should not be done, perhaps particularly if the concept of a minimal procedural grammar proposed here were to be interpreted in stratificational terms. What systemic grammar may need to learn from stratificational grammar, it seems to me, is more about ways of handling interstratal discrepancy, and in particular about setting limits to complexity of this type. Thus, our grammar of the traffic light code can be regarded as a simple systemic - and perhaps stratificational - grammar, and the expanded model with all the additional complexities that I have just suggested for language is also a systemic grammar. They are in overall terms the same semiotic model.

Halliday may be fairly described as the principal architect of systemic theory, but behind him stands Firth, as Halliday himself often acknowledges. What I hope to have shown here is that the line goes directly back even further, to Saussure, and that almost all of the essential concepts in systemic theory are to be found, if only in embryonic form, in Saussure. The corollary of this is that, if you accept Saussure's view that 'to discover the true nature of language we must learn what it has in common with other semiological systems' (1916/74:17), you may wish to consider (or reconsider) systemic theory as a candidate for further examination and further development.

This is all very well, you may say, but I want to see what a description of a language that is based on these theoretical concepts looks like. While Saussure himself did not leave us the example that we might have asked for, he did offer the advice (p.137) that:

each fact should be fitted into its syntagmatic or associative [paradigmatic] class, and the whole subject-matter of grammar should be arranged along its two natural co-ordinates; no other division will show what must be changed in the usual framework of synchronic linguistics. I cannot undertake that task here, for my aim is limited to stating only the most general principles.

Like Saussure, my aim in this paper has been to state 'only the most general principles'. However, I have gone one small step further, in that the most basic points have been illustrated in our explicit grammar for the traffic light code. And in my book Cognitive linguistics and social interaction (1980) I have tried to show what a considerable portion of a model of language constructed on these principles would be like.

Finally, it is perhaps fair to point out that it is only in what are still minority theories such as stratificational, tagmemic and systemic grammar that we find an answer to Saussure's call to change 'the usual framework of synchronic linguistics' in a way that gives adequate recognition to both syntagmatic AND PARADIGMATIC relations - and that at the same time offers an overall framework for the level of SEMANTICS.

5.2 Envoi

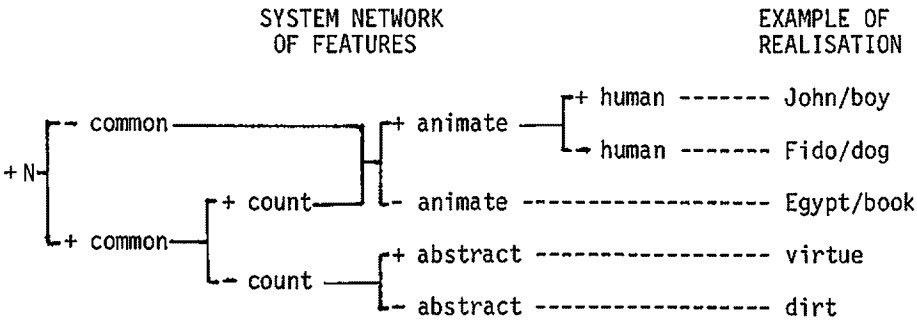
After completing the first version of this paper I came across a hitherto unread passage in Jonathan Culler's Saussure that gave me a particularly powerful experience of the feeling that one sometimes receives on discovering that someone else has had an idea and found words for it that are almost identical to those that one has arrived at for oneself. Culler writes (1976:88):

While other Saussurean concepts have been assimilated [by linguists], Saussure's ruling concept, the notion of the sign, and language as a system of signs, has largely been neglected. Linguists have paid lip service to the concept but have not allowed it to govern their analysis of a language. It can be argued that if the sign were granted the role it has in Saussure this would lead to an important re-orientation of linguistics, but until the attempt is made one cannot say what the consequences would be.

This paper, together with Fawcett 1980, constitutes one such attempt.

Will linguists ever come to agree on the essential nature of language, as geologists have come to agree on the essential nature of erosion? Will Ferdinand de Saussure's 'ruling concept of the sign' be accepted just as the earlier concept of erosion proposed by Horace Bénédict de Saussure has been? Will linguists allow it to govern their analysis of a language, as Culler suggests they should, perhaps in some such way as that proposed here? I am confident that not everyone will agree with every aspect of what I have said, and some may still remain in fundamental disagreement on certain matters. But my own belief is that we must accept the need for a model constructed around the concept of the sign - and so round a mutually defining semantics and form, related by explicit realisational relationships - if we are to establish a sufficiently common basis among our complementary theories to enable us to discuss with real insight those matters of detail that are, paradoxically, the only evidence for those grand, overarching concepts we have discussed here.

APPENDIX A
SOLUTION TO THE PROBLEM POSED IN SECTION 3.6



A more elegant representation of Figure 9

APPENDIX B: CHECK LIST

SAUSSURE		RE-INTERPRETATION	CONCEPT PRESENT IN THEORY?			
			1:	2:	3:	4:
Part 1: <u>Delimiting the object of inquiry</u>						
1 synchronic vs. diachronic		synchronic vs. diachronic				
2 langue vs. parole		grammar (potential) vs. output (actual)				
(holistic) (psych. reality)		grammar should be a) explicit b) holistic c) model of soc-psych. reality				
Part 2: <u>The shape of a semiotic system</u>						
3 signifier vs. signified (signification)		semantics (signifieds) form (signifiers) realisation				
4 paradigmatic vs. syntagmatic		paradigmatic vs. syntagmatic				
5 substance vs. form value		non-code a) knowledge of the universe b) (phonetic) substance vs. code value				
Part 3: <u>The extra complexity in language</u>						
semantics: syntag. - in grammar - in outputs paradig. - relations between features - in grammar - in outputs - rank - in grammar - in outputs - functional plurality - in grammar - in outputs						
form: syntag. - in grammar - in outputs paradig. - in grammar - in outputs						
intonation: syntag. - in grammar - in outputs paradig. - in grammar - in outputs						
segmental phonology: syntag. - in grammar - in outputs paradig. - in grammar - in outputs						

Question: Is the best theory necessarily the one with the most ticks in the boxes?

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II

GENERAL LINGUISTIC THEORY

ON UNDERSTANDING PEOPLE--AN INTEGRATIVE PHILOSOPHY
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1. Person, in Society, above Abstraction or Idea

In this paper I am affirming--not "proving"--that as a basic metaphysical principle we would do well to take the individual person as more basic than abstract idea. For three centuries or so the dream of the academic community seems to have been to become "scientific." And science, as an ideal, seems to have been defined as being expressible in mathematical terms. But mathematical notation is highly abstract; the phrase two plus two equals four does not of itself mention apples. Yet if the formulas of linguistics do not of themselves mention people or their intents, dreams, or fears, people may seem to melt away and disappear. Linguistics may thereby become an autonomous discipline --but with an autonomy in which man (or woman) is separated from speech and from utilization of that speech in behavior. Fortunately, an inevitable reaction has begun to set in. Sociolinguistics begins to treat man in his social environment, and psycholinguistics begins to treat language in the environment of man's creative mind.

2. The Particular as Important

In the process of mathematization, formulas and concepts get more and more general. The particular tends to get lost. Eventually the individual person may seem unimportant. And, at the same time, single innovative changes or departures from a norm may not be captured by the mathematicized rules--rules which are in the process of being changed by these particular aberrations.

The academic climate often has included a philosophical reductionism in an attempt to explain every complex in terms of that which is simple. The atom may then appear, in some sense, as more significant or basic than the brain of the physicist in which it exists. Such a philosophical reductionism may be unable to appreciate the fact that the particular, or the atom, is relevant only in a larger context. Yet eventually life as lived must embrace the particular as a point in a pattern of the general. Both must be maintained, particular along with pattern which it exemplifies.

3. The Development of Integrative Philosophy

Particulars are parts. Parts are in wholes. Wholes continue upward until one reaches the top of one's metaphysical non-provable starting point. Hierarchy must be as basic in our thinking as reductionism, and high level units as basic as low level ones--or even more so. Somehow we must be able to find a way to show that a derivation or prediction of the small from the analytical breakdown of the large is just as valid *emically* as is an understanding of the large in terms of a pattern of various small elements. In this way the linguist can identify himself as a basic element in the universe. Autonomous syntax, autonomous linguistics, and the autonomous linguist all disappear. Even the autonomous object--existing "out there" as a "thing-in-itself" apart from any human or theistic observer--disappears from our ken. Rather the universe as talked about is a whole, integrated. And philosophy must develop to handle the implications of such presuppositions.

I feel that in my tagmemic view of human behavior, including its hierarchical structure, such an integration has already begun to be developed. In my Language (first edition in volumes 1954, 1955, 1960, with second edition 1967) I showed, for example, that many events include both verbal and nonverbal materials. A football game does so, since one must include the verbal rulings of the officials. A breakfast scene or a church service is likewise an integration of much more of life than language alone, or nonverbal behavior by itself.

Recently Robert Longacre called my attention to a book by Pepper on World Hypotheses; Pepper emphasizes ([1942] 1970:84-114) that a basic world view must start from a root metaphor. Such a metaphor eventually ties in to basic observations of common sense, which are then developed, tested, and modified to cover more and more empirical data and general notions of types implied by that data. The change from one root metaphor to another leads to a different philosophy of life--to a different view of the universe with its persons and objects.

What would happen if one took the basic principles of tagmemics, and extended them into the area of metaphysics? Would philosophers approve of current tagmemic presuppositions? Or would different philosophies support certain of these presuppositions but reject others? Or could the tagmemic start, which has an empirical base in the analysis of many different languages, suggest a new philosophical synthesis? I will suggest that it can do so, and that it will lead to an integrative philosophy (a term which I have coined to name and to characterize my approach).

It is not appropriate here to attempt to discuss these principles in detail. Perhaps the easiest way to see them, at the moment, is in my article of 1980 'Here We Stand' which lists a dozen of these principles, and illustrates them with notes about a hypothetical trip to Pluto. In a second article (1981) I

discuss "talked-about" reality, in relation to scholars from Heraclitus to Polanyi, Bridgeman, Samuel, and Einstein. But the most serious attempt to lay this groundwork is in a book which I have written for nonlinguists (illustrated heavily with linguistic data, however) which is scheduled to be published this fall: Linguistic Concepts--an Introduction to Tagmemics. In this book (as in the first of the two articles just mentioned) one can see discussed and illustrated the concepts necessary to allow for emic units (utilizing observer-to-thing relationships, but not allowing life to be made up exclusively of relationships); the concept of hierarchy (but requiring at least three hierarchies for language--phonology, grammar, and reference); the concept of context (including the necessity of keeping form and meaning tied closely together, the relation of context to change, and to a universe discourse); as well as observer perspective (static, dynamic, and relational--or particle, wave, and field). (See, also, my discussion in LACUS Forum 1978, and Tagmemics 1981.)

4. Some Persons Related via the Four-Cell Tagmeme

In Figure 1, I give labels for the four cells of the tagmeme: slot, class, role, and cohesion.

SLOT		CLASS
Syntagmatic		Paradigmatic
Wave		Manifested unit
Position		Particle
	1	2
ROLE	3	4
Relevance		COHESION
Impact		Relational
Meaning		Pattern control and agreement
		Background field
		Frame of reference

Figure 1. The four-cell tagmeme has features of position, item, structural meaning, and frame of reference.

Since a slot is a structural place in a larger, hierarchical structure, and since the sequence of slots in an immediately larger structure then comes under attention, we might label the slot cell as having a syntagmatic emphasis. On the other hand, the audible or visible manifesting materials which form a class, and which can fill that slot in the syntagmeme, may--because of their replacing characteristics--be called paradigmatic. Role, on the other hand, emphasizes the impact, or relevance, or semantic component of one of the units of the hierarchies of phonology, or of grammar, or of reference. The fourth cell, which

I have called cohesion, refers to a pattern of elements either tying items together in a sequence, or as relating them to background explicit or implicit structures; so it could perhaps be treated as referring to a pattern, or relational system.

Figure 1 also shows that an element in the slot cell can be called wave, inasmuch as that element may frequently be identified as the nucleus or as the margin of a containing structure (with a nuclear element tending to be more in focus than a marginal one). The elements of a class are sometimes treated as segmental chunks, and hence may be called particles. Cell four, referring to the background structure, and to its control of agreement, may be called a field.

Can we now use this element of tagmemic theory to help us to understand more deeply certain of the components--not, of course, of the total outlook--of the contribution of some scholars to us? In some of the clause notation or description of Jespersen ([1964] 1969:99) subject, object, and verb (predicate) are emphasized; since the subject and object notions are used by Pike and Pike ([1977] 1982) in the slot cell for notations about clauses, I have put Jespersen in Cell 1 of Figure 2. Fries, on the other hand, I have put in Cell 2 of Figure 2, since in his notation for clauses (1952:76-85) he uses units such as Class 1, Class 2, Class 3--which, in general, include nouns, verbs, and adjectives respectively.

Jespersen	1	2	Fries
	3	4	
Bloomfield			Lamb
Fillmore			Martinet
			Halliday and Hasan

Figure 2. Certain scholars, in notations or descriptions which they sometimes use for representing clause or other structures, may have an emphasis which can partly be represented in terms of the four-cell tagmeme. Such an approach, with an integrative tagmemic philosophy pulling them together into a single coherent whole, may help us to learn from and enjoy different theoretical perspectives without loss of some of their individual important emphases and contributions, and without disintegration into a noncoherent eclecticism.

Bloomfield (1933:173, 267), on the other hand, in naming certain grammatical constructions, mentions types such as actor-action or goal-action. This is clearly related to the grammatical semantic component of these tagmemes, so I have put Bloomfield in Cell 3. But what an irony! These particular labels are themselves semantic, although people often seem to think of

Bloomfield as being nonsemantic in his approach. Similarly, the labels for the cases of Fillmore (1968:24-31) would seem to come here. On the other hand, Lamb (1966:3) emphasized language as a system of relationships which are not directly observable. These relationships do not seem to fit within the concrete classes of Cell 2, nor with the constituents of the wave of Cell 1, nor with the semantic components of role of Cell 3. So we treat these relationships as related to the patterns, or system, of Cell 4. Some of the work of Martinet I would put there, also, because of his strong contribution to the use of a phonetic chart in discussing phonological systems: he has pointed out that a gap in a chart is more easily filled by borrowing than is the isolated extension of a line or a column. Halliday and Hasan, on the other hand, emphasize cohesion within a text (1976:329-55) by means of a 'tie' which is 'a relational concept'.

I personally find this hyperbole of mine, dealing with clause notations, useful. I am able by it to help students gain an initial, partial understanding of people by seeing some of their sample emphases. Of course one must be careful. Bloomfield, for example, dealt with noun classes--not merely with cases. One must be similarly cautious about categorizing other people from just one point of view. And some scholars do not fit this particular four-cell array easily, but are better treated in relation to other components of tagmemic theory. Chomsky, for example, is not easily categorized in relation to any one of these four categories. When, however, one takes the tagmemic principle that insists on maintaining at all levels, in all hierarchies, the form of a unit and the meaning of a unit in an immediately available composite, then Chomsky in his early work (1957:17, 92-105), in contrast with tagmemics, may be characterized as dealing with an autonomous syntax with its form abstracted from its meaning and from its physical output or performance variations.

In addition, if one looks at a rule structure as a statement of what the linguistic reader of the rule does to get from the left of an arrow to the right of it, in the metalinguistic representation (not in the statement of actual language events), then a rule might be viewed as a metawave (not as a data wave).

5. Person-in-Things--The Emics of the Universe

I want, in integrative philosophy, a person to be given priority above things, and a person to be treated as having relevance because of his distribution in a society which he in part makes and comprises, and which in turn affects him and in part builds him. And I want the particular individual to be treated as relevant, as having intents and purposes necessary to his individuality in a society.

Choice of perspective must also be available to persons, if we are to have the freedom to live normal lives or make our own contributions to science from our preferred viewpoints--as, for example, is implied by Figure 2. But where does such choice fit

in tagmemics, as part of the integrative philosophy proposed here? On the one hand, the observer (whether the man-in-the-street or a scientist) can choose to see the world as made up of discrete events, with beginning and ending (i.e. as particles), or he can notice that the joints between events may be indeterminate or may be overlapping (i.e. events seen as waves), or he can look at a set of events as each being a point in a larger including pattern (i.e. as points in a field). For other purposes, or at other times, he may combine two or three of these perspectives in some kind of ranking of priority (as discussed in Young, Becker, and Pike 1970:122-23); for example, a house (particle) may be seen as being finished (wave), with a nice ground plan (field).

In addition, the observer enters into all emic orientations--all perceptions where the social structure determines the normal individual's selecting and grouping into attention of objects (Sinclair 1952) or of nouns and phrases, or of sounds and syllables, or of persons and episodes. The aspirated and unaspirated allophones of English /t/, for example, enter a single emic unit. Emic unitizing implies observer involvement. Perception involves emicizing in some fashion. So no objects as talked about are totally unaffected by observer relations to them via individual biography or via indirect community impact on the individual.

6. Work to be Done on Integrative Philosophy

If such a proposed philosophy is to be useful, more research must be done on it. I am pondering something of the following, for the next stage:

Can a simplified taxonomy of philosophies of the past be developed in such a way that one can ask which of the tagmemic principles have been used by it, and with what strengthening thereby? Which principles are omitted, and with what cost?

In discussing this with anthropologist Elinor Abbot, she urges that I not overlook the context in which philosophies have developed in the past. What was the social structure of the time? How could the progression of changing philosophies be viewed as a continuing dialogue, with comment and reply? This would seem to her more congenial to tagmemic theory than a study of philosophies abstracted from their social-historical settings. Here, also, I must do the library research, which is only just under way.

In the meantime, however, note that Donald Davidson, a former president of the American Philosophical Association, affirmed in 1979, in an article on 'The Method of Truth in Metaphysics,' that 'In sharing a language, in whatever sense this is required for communication, we share a picture of the world that must, in its large features, be true. It follows that in making manifest the large features of our language, we make manifest the large features of reality. One way of pursuing metaphysics is therefore to study the general structure of our language' (1979:294), and 'that by studying the most general aspects of language we will be studying the most general aspects of reality. It remains to say how

these aspects may be identified and described' (295). In tagmemics I have for years tried to identify such highly general aspects. It now remains to try to determine whether or not these capture some, at least, of 'the most general aspects of reality.' If philosopher Davidson is even partially right, linguists owe this work to the world.

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BLOOMFIELD'S FUNDAMENTAL ASSUMPTION OF LINGUISTICS

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1. Incompatible goals

The first and main point of this paper, which I should like to state in advance, is that linguistics cannot be disciplined simultaneously by grammar and by science. Grammar and science are incompatible.

By science I mean nothing more than the ordinary concept of science as found in physics, chemistry, and biology. The goal that linguistics should be scientific was introduced a century and a half ago by the founders of modern general linguistics, and today linguistics is standardly defined as the scientific study of language.

By language I mean nothing more than the ordinary sense of language as understood in linguistics. Language is a sign relation between sound and meaning given by grammar and lexicon. It is in this sense that linguistics as the study of language is disciplined by grammar.

Now I shall try to explain why it is true that the goal of linguistics to study language and be disciplined by grammar is incompatible with the goal of linguistics to be disciplined by science, and thus why it is that current linguistics, which accepts these two incompatible goals, is incoherent at the core. Then we can consider what should be done about it.

2. Science

In order to understand why grammar and science are incompatible we need to recall how the individual sciences have crystallized out of philosophy.

The ancients divided philosophy into three parts, the physical, the ethical, and the logical. Science has emerged from the physical part, the part dealing with nature, with our understanding of physical reality. It has involved giving up older false theories and unexamined assumptions about nature inherited from folk theory, mysticism, religion, inspired but wrong guesses and the like; and replacing these by sounder theories that better correspond to available observational evidence. In science when confirmed observations are found to conflict with existing beliefs, assumptions, or theories, it is theory that must give way, no matter how deep our belief in it or our commitment to it may have been.

But apparently not all assumptions unsupported by observational evidence can be eliminated. We need to distinguish between

theory and metatheory. Whereas scientific theory can be subject to test against the evidence, methodological assumptions about the conduct of science itself, about the proper construction and testing of theories, are in the realm of metatheory, part of the theory of knowledge, and are not necessarily testable against the evidence. These concerns, although they are of interest to scientists, stand outside of science proper and remain as part of philosophy.

The metatheoretical nature of these concerns was already clear to the Stoics of the third and second centuries B.C. when they put these matters in the logical part of philosophy as distinct from the physical part. They realized that the logical part was prior, for without it the physical part and the ethical part could not express themselves (Diogenes Laertius VII 39, 40, 83) (Hicks 1925). The criterion of truth about nature in the physical part was clearly understood to be observation through the senses. But in the logical part some appealed to right reason, others to innate ideas or gifts of nature, a controversy that is still with us (Diogenes Laertius VII 46, 49, 50, 54).

Modern science has advanced by seeking out and eliminating as many unsupported assumptions as possible. It retains only the following four.¹ There is an ontological assumption that there is a real world out there that can be observed and studied from different points of view. It's not just an illusion. There is a regularity assumption that nature is coherent, that there is a possibility of finding out something about it. There is a rationality assumption that we can reason to correct conclusions from correct premises. And there is a causality assumption that observed phenomena result from immediate causes -- where there's smoke, there's fire.

These metatheoretical assumptions of science agree with good common sense. Their credibility is enhanced by the many successes of scientific inquiries based on them. No science has additional special assumptions of its own, not at the meta level, and especially not at the level of its special subject matter. Everything else in science beyond these shared assumptions is theory or hypothesis based on observational evidence and testable directly or indirectly against observational evidence. This is the deep meaning of the unity of science -- common assumptions and a common method.

3. Linguistics

When linguistics started to become a science a century and a half ago it inevitably found itself in a different position from the physical and biological sciences. Its subject matter and prescientific lore did not stem from the physical part of philosophy but from the logical part, the ancient metatheoretical part, and this soon became a source of problems for a would-be science of language.

In fact the ancient intellectual foundations of modern linguistics can be seen most clearly as part of the Stoic dialectic or theory of knowledge. The Stoics related signifier, signified,

and referent in a sophisticated theory of signs.² The signifier was the sound of the voice. It was related to the signified, the lekton or logical deep structure, through phonetic, phonotactic, and syntactic levels. There was a relation of signification between elements at the syntactic level and elements in the logical deep structure, which also contained propositions, questions, active and passive, cases, and other semantic constructs. This logical deep structure was related to the referents in the real world through levels of perception and sensation. The whole structure represented an account of how we know the truth about the world and how we say it.

The part of this structure that related signifier to signified was transmitted to modern linguistics through the unbroken tradition of normative grammar. When the Greeks developed an institution of normative grammar after the conquests of Alexander the Great, Stoic dialectic provided its intellectual underpinnings, and Alexandrian classical scholarship provided a tradition of textually-based research. Stoic philosophy and this type of grammar were influential in Rome, and the combined Graeco-Roman grammatical tradition gave rise to the normative grammar of modern European nationalism. Then the founders of modern scientific linguistics rejected prescription in favor of description, and favored statements based on what people actually say or write rather than on philosophically or logically based preconceptions. Yet they retained virtually without question the conception of language and grammar that they had inherited from ancient philosophy through normative grammar.

And since this conception of language and grammar had descended not from the physical part of philosophy, but from the logical part, not as part of what we know about the world, but as part of how we know it, it inevitably ran into the same questions concerning the criterion of truth in the logical part of philosophy that had worried the Stoics, for it involved assumptions that could not be tested against evidence from the senses. As a legacy of this descent, modern linguistics finds that it can not always base its claims on observation. Much of the confusion and contention in modern linguistics stems from this fact, for lacking objective criteria, there is a temptation to rely on intuition, arbitrary dicta, partisanship, murky polemics, and intellectual bullying.

4. Saussure and Bloomfield

Saussure gave us a hint of the source of these problems when he exclaimed that "Other sciences work with objects that are given in advance and that can then be considered from different viewpoints; but not linguistics ... Far from it being the object that antedates the viewpoint, it would seem that it is the viewpoint that creates the object" (1959:8). That is, other sciences work with objects of nature like rocks, plants, and animals that can be considered from various viewpoints in physics, chemistry, and biology. One can always go back to the object, which is given in advance of theory, and test one's theory against further observa-

tions of the object from the same or different points of view. But not linguistics.

In terms of the parable of the blind men and the elephant, the blind men, who have each examined different parts of the elephant and obtained quite different views of it, can always go back to the elephant and eventually come to agree and to see it as a whole. But in linguistics there is no elephant, and linguists cannot agree.³ The objects of language are not given in advance in spite of the common illusion that they are.⁴ They are simply created by differing points of view.

It is to Bloomfield that we owe a further insightful development.⁵ Bloomfield pointed out that in a human society individuals cooperate by means of sound waves (1933:28). But we find in phonetics that the sound waves of speech are infinitely complex and infinitely varied, and that no two utterances are exactly alike (1933:76). In order to move to phonology one has to recognize successive utterances of the same speech-forms as being the same, and this requires knowing the meaning. Bloomfield shows this by showing that different utterances of the word *man* in different pitch-schemes are the same in English but different in Chinese.

Then he said that since we do not have a scientific control of the meaning, phonology, and with it all the semantic phase of linguistics, rests on an assumption -- the fundamental assumption of linguistics -- "in every speech-community some utterances are alike in form and meaning" (1933:78). This assumption introduces into linguistics just those objects of study that are not given in advance. "In every speech-community" introduces the concept of language since a speech-community was defined as those who speak the same language. "Some utterances are alike" introduces a segmentation both on the syntagmatic and paradigmatic axes that is not inherent in the sound waves. "In form and meaning" associates these segments with signs and their forms and meanings in the traditional sense. Bloomfield had said that to study the coordination of certain sounds with certain meanings is to study language (1933:27).

Thus Bloomfield took pains to embody in a single fundamental assumption all the assumptions necessary to create just the objects of language that Saussure had pointed out are not given in advance. And in so doing he ratified the nonscientific nature of linguistics at its core.

But one may object that we really can study speech-sounds scientifically, and it was linguists like Bloomfield who showed us how through methods of sameness and differences. Yes, one can be empirical down to a point, but at the core one runs into a special subject-matter assumption. If one were to grant the assumption that there are ghosts and goblins, one could try to distinguish empirically between the two by listening to the sounds heard in an old abandoned house. But such a study would not be scientific since it rests on an unsupported assumption.

Thus we see that our first point is really true. Linguistics can not study language and be disciplined by grammar, and simultaneously be disciplined by science, because the objects of language

are not objects of nature given in advance. They are merely created by a point of view. In fact they are introduced into the discipline by an observationally unsupported assumption, Bloomfield's fundamental assumption of linguistics or its equivalent. The objects that constitute language are simply ghosts of a pre-scientific era.

The most salient fact of present-day linguistics is this fundamental incompatibility of grammar and science. It renders business as usual in the discipline highly suspect, to say the least.

5. Psychological and social reality

Let us now turn to another important point. In addition to the goals of linguistics to be scientific and to study language, a third longstanding goal has often been accepted. This is the goal of seeking explanations of linguistic phenomena in terms of people. Since the other two goals are incompatible one wonders about the status of this goal.

A goal of explaining, understanding, or interpreting linguistic phenomena in terms of people is currently attested in Chomsky's (1965) talk of grammar as reflecting the competence of native speakers, and his so-called innateness hypothesis. It is attested in Lamb's (1971) concept of cognitive networks and Halliday's (1973) concept of the child's internal model of language. And it is attested in my (1960) hypothesis of a relation between syntactic structure and the limited capacity of human temporary memory.

These and other familiar examples that could be cited all attempt to relate language and grammar to people. They all rest on an assumption of the psychological or social reality of grammar. Linguistic phenomena have first been analyzed in grammatical terms, which are based on an untestable assumption of objects existing in the metatheoretical domain of language and logic. Then these results are assumed to constitute a theory of the psychological or social reality of people as part of the real world. This is a fallacy because language and people exist in different domains entirely. It is a fallacy to confuse metatheory with theory, philosophy with science, logic with psychology, or a theory of knowledge with a theory of knowers.

Thus the goal of linguistics to seek explanations in terms of people is incompatible with the goal of studying language. But it is not incompatible with the goal of being scientific because people certainly are given in advance and indeed can be studied scientifically.

6. The two alternatives

Since the first goal, that linguistics should study language through grammar, is incompatible with the other two goals, that it should be scientific and seek explanations in terms of people, one cannot escape the conclusion that current linguistics, which commonly accepts all three goals, is incoherent in its foundations.

One is driven reluctantly to the conclusion that if the discipline is to retain its integrity it cannot escape considerable restructuring of its goals, assumptions, foundations, and methods.

What is to be done about it? There are only two coherent and viable options open to us: a linguistics strictly at the level of metatheory that accepts the first goal, or a linguistics strictly at the level of science that accepts the second and third. The current mixed linguistics is incoherent.

The first alternative would retain only the goal of studying language. This would yield a philosophical linguistics of pure grammar in contact with a pure logic in a theory of knowledge. By abandoning the goal of being scientific it would insulate its theories from being tested against mere data, for that would lead to a rejection of its fundamental assumptions. And by abandoning the goal of seeking explanations in terms of people it would avoid the psychological and social reality of grammar fallacies. It would be a normative and prescriptive discipline focusing on a metatheoretical ideal of perfection. If theory and observation were found to conflict, this alternative would favor theory over observation, attributing the difference to performance errors and the unfortunate propensity of mere mortals to think illogically and speak ungrammatically.⁶

The advantage of this alternative is that much is already known about logic and grammar. But it would be a move backward into philosophy, nullifying the halting steps that linguistics has already taken toward science.

On the second alternative linguistics would retain only the goals of being scientific and of seeking explanations in terms of people. By abandoning the goal of studying language it would eliminate unsupported special assumptions and avoid the psychological and social reality of grammar fallacies. It would be disciplined by science rather than by grammar, and this would necessitate considerable restructuring of foundational theory. It would be a human linguistics that studies people rather than language, seeking a scientific understanding of how people communicate. It would focus on scientific explanation rather than prescriptive rules, and its theory would account for the evidence and be testable against the evidence. If theory and observation were found to conflict, this alternative would allow theory to be corrected by observation, hoping thereby to achieve a better scientific understanding of people as objects of nature.

The advantage of this second alternative is that much is already known about science. It would be a step forward by which linguistics might finally achieve the destiny long foreseen for it as the key science linking the physical and biological sciences to the psychological and social sciences and the humanities.⁷

It is important to realize that the differences between the first and second alternatives are immense compared to the small differences between the many contending versions of grammar. This choice cannot be fudged by an eclectic or ecumenical position. Any attempted compromise is doomed to incoherence. Basically it's

a choice between following theory or following observation when the two conflict.

The most important choice that we face is this choice of moving back to philosophy or moving forward to science. I predict we will choose to move forward.

7. Human linguistics

This second alternative has engaged my interest for a number of years, but the approach is so new and different that much remains to be done.⁸ This is not the place to review that work, but I can explain how Bloomfield's insights have helped with a central conceptual issue.

Recall that Bloomfield used a method of recurrent sames and differences to establish phoneme inventories and other details of the structure of language. But he said that the difference between distinctive and nondistinctive features of sound lies entirely in the habit of the speakers. Bloomfield did not go on and take the distinctions made by speakers as evidence about the habits of the speakers. Instead he used these observations to work out structures in an assumed domain of language, and then tried to resist the temptation to attribute psychological reality to the resulting grammar.

But in human linguistics we can use the distinctions made by native speakers directly as evidence about the linguistic structure of the speakers themselves. In this way linguistics can be scientific and seek explanations in terms of people. The price we pay is that this linguistics cannot be disciplined by grammar. It would have to be structured along scientific lines from the ground up.

This restructuring poses the most urgent and exciting challenge that the discipline has faced in modern times. It promises to remove the underlying causes of the intellectual fragmentation and sectarianism currently afflicting our chosen discipline. To carry it out will require the clearest thinking and the most careful work that we are capable of.

NOTES

¹ This is meant as a rough characterization of the metatheoretical assumptions accepted by practicing scientists in the physical, chemical, and biological sciences. If what scientists actually do is different from what philosophers or logicians think they do or think they ought to do, we will be referring to the actual practice of scientists when we speak of science.

² Various versions of such a three-membered semiotic theory can be recognized in the works of a number of influential philosophers from ancient times through the middle ages and right up to today. See Aristotle *On Interpretation* 1. A conspicuous modern example is the semiotic theory of Charles Sanders Peirce. The

Stoic version is of central importance for the history of linguistics. The account in Diogenes Laertius VII contains what may be the earliest known grammar in the Western tradition. A linguist studying the convenient parallel translation in the Loeb edition (Hicks 1925) should check the Greek for all technical terms as they are not always translated consistently. See also Arnim (1903).

³ The idea of using the elephant parable in this connection was suggested by Martha Ratliff.

⁴ In his unpublished notes Saussure exclaimed that the illusion of things *naturally given* in speech is profound (Engler 1967:24-26).

⁵ Introduced in Bloomfield (1926).

⁶ The rationalistic and Cartesian movement in linguistics under the influence of Chomsky and others with a philosophical bent would pull linguistics away from science toward the first alternative, but such efforts are typically mixed and therefore incoherent.

⁷ The potential key position of linguistics among the sciences has been hinted at or expressed many times. See, for example, Sapir (1929:212), Bloomfield (1939:55), and Hockett (1948).

⁸ Selected progress reports include Yngve (1969, 1975, 1981a, 1981b).

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C C FRIES, SIGNALS GRAMMAR AND THE GOALS OF LINGUISTICS

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In the last twenty years a mythology has developed which asserts that the structural linguists of the thirties, forties, and fifties all had the same model of language and goals of description. There is some justification for this myth, for all structuralists believed with Sledd that "a language . . . is first of all a structure, a system, an elaborate set of patterns: and a system can only be described systematically." (1959.7) However, if we carefully examine the goals of the structural linguists of the time, several traditions emerge, of which I wish to focus on two opposing ones.

The first tradition emphasized discovering and describing the patterns and structure of the language. Perhaps, of all the structuralists, Harris expressed this goal most clearly when he said:

Descriptive linguistics . . . is a particular field of inquiry which deals not with the whole of speech activities, but with the regularities in certain features of speech. These regularities are in the distributional relations among the features of speech in question, i.e. the occurrence of these features relatively to each other within utterances, (1951.5)

Similarly, Bloch and Trager described their goals for phonemic analysis in the following terms:

Our object . . . is . . . the practical but no less scientific one of ordering the sounds of a language into a minimum set of contrasting units, in terms of which the pronunciation, the vocabulary, and grammar of the language can be most efficiently described. (1942.44)

Underlying these statements seems to be an unstated assumption that all patterns and regularities in the distribution of features of speech are important. Thus, when A. A. Hill found that some native speakers of English could not perceive the four degrees of stress which they themselves produced, he concluded that "there are speakers who have four contrasts, but who still have difficulty in hearing all the distinctions they make." (1958.18) In other words, the pattern (the contrast) is important, whether or not the speaker reacts to it.

Transformational grammar fits squarely within this tradition, for Chomsky, a Harris student, accepted the description of patterned regularity in language as his major goal of description, though his notions of what constituted an adequate description were considerably more complicated than those of his predecessors. He described his goal in the following words:

The fundamental aim in the linguistic analysis of a language L is to separate the grammatical sequences which are the sentences of L from the ungrammatical sequences which are not sentences of L and to study the structure of the grammatical sequences. (1957.13)

Like the earlier statements within this first tradition, the goal described here is fundamentally a taxonomic goal. It is the classification of sequences into one of two classes, sentences or non-sentences. Further, the tool by which this goal was to be achieved was a sophisticated account of the distributional properties of the various morphemes and constructions in the language. (Part of the sophistication, of course, lay in the fact that not every relevant element had to occur in the final string.) While later statements of the goals of linguistic analysis within this first tradition place less emphasis on taxonomy and place greater emphasis on relating language form to language meaning, these goals remain essentially static and at least imply a taxonomy of sequences into sentences and non-sentences as a major tool of the analysis if not actually a goal of the analysis.² For all these linguists, the result of writing a grammar is a description of a class of items called sentences, together with a description of the meanings of these sentences. Such descriptions using various criteria for the classification and labeling of elements within the sentences (or, in later years, within their deep structures).

This approach has several consequences. First, obviously, since the goal of such a grammar is to describe a class of items, the sentences of the language, a crucial focus of the investigator necessarily must be on what are the members of that class. Hence, particularly with the beginnings of generative grammar, linguists came to focus on questions of 'Is this sentence grammatical?' with all the attendant arguments over the presence or absence of asterisks and "my dialect".

Second, the criterion for success of a grammar was the criterion of the generality of the patterns discovered and the simplicity of their description. This criterion was frequently used informally in the forties and fifties; one can detect its presence in the Bloch and Trager quotation given above where they talk about "efficiency" of description.³ However, with the advent of generative grammar the concept of simplicity was elevated into a formal criterion for evaluating alternative grammars.⁴

Third, since the linguists of this first tradition focussed

on regularity, and since no two individuals speak alike, linguists in this tradition sought, in theory at least, for the place where the greatest regularity was to be found, the language of the individual speaker. Thus the concept of idiolect was a natural development of this tradition and was well established by the nineteen fifties. Again, transformationalists accepted this concept and, via the notion of the "ideal speaker-hearer" (Chomsky 1965.3) elevated the idiolect to the status of language.

Finally, a further natural, if not inevitable, result of the combination of the search for regularity in language with the emphasis on simplicity of explanation is the increasingly abstract nature of the linguistic descriptions. In phonology, first the phoneme was discovered, and then phonemic features, long components and the morphophoneme. Within morphology, linguists quickly learned the uses and misuses of zero. In syntax, linguists began by describing the sequences of forms that actually occurred in each sentence, (this was taxonomic linguistics in the Chomskian sense) but then moved to relating different constructions via transformations, and later to positing ever more abstract deep structures.⁵

Let me now turn to the second tradition within structural linguistics. This tradition takes as its goal the description of those features present in the language (or in the context in which the language is being used) which serve as signals of the meanings being conveyed to some group. Bloomfield seemed to adopt this goal when he said:

To describe the grammar of a language, we have to state the form-classes of each lexical form, and to determine what characteristics make the speakers assign it to these form classes. (1933.266)

C. C. Fries put it more generally when he said.

[structural] grammar aims not at definitions and classifications but at such a description of the formally marked structural units as will make possible a valid prediction of the regular recognition responses that the patterns will elicit in the linguistic community. (1967.668)

(He gave a more extended description of this goal in points 5 through 9 in the statement reprinted in the appendix.) Here the goal is not one of classifying items as sentences nor is it one of describing the patterns in or structures of various units. Rather it is one of predicting how speakers and listeners will perceive and interpret what is said. Since Fries held an extreme and unambiguous position on this issue, I will henceforth focus on his work, and indeed on his later work in which his statements are most clear.⁶

Two points are worth emphasizing about Fries's approach here.

First, describing the structures of the sentences of the language was not a goal for Fries, but rather a means toward his overall goal. Second, his wording emphasized listener reaction rather than speaker behavior. (Note that he wished to predict recognition responses, not the sentences of the language.) I believe that these two points have far reaching consequences for the way Fries did linguistics. First, the deemphasis of the importance of structure and of distributional regularities led him to deemphasize the formal identity of elements. For him, the particular physical nature of the signal of meaning was less important than its contrastive nature and its function as a signal. Thus these signals are "bundles of contrastive differences--contrastive differences of sound features, of sequences, of distribution, of pitch. They are abstractions." (1963.78) In phonology he said that "purely phonetic considerations should not (must not) be used as determining factors in establishing the phonemes of the language." (1959.5)

Even regularity of patterning of phonetic phenomena was not sufficient for Fries to establish contrast. Thus, when discussing Sledd's analysis of English in a letter to me he said:

When I heard Sledd arguing for his so-called "tenth" vowel in English, it seemed to me that nearly everything he offered as evidence was based on phonetic considerations. I didn't dispute his assertion that he always made the distinctions he said he did But what I wanted to have was some evidence of the existence of some community, no matter how large, in which that particular contrast of sound features actually made such a difference in the speech signal that the responses regularly elicited differed regularly. It isn't enough, I believe, that, for the speaker . . . the opposition regularly occurs, or even that it regularly occurs for him in correlation with recurring "sames" of stimulus features. It must, in addition, I believe, regularly elicit recurring "sames" of response features in some community. (1959.5-6)

Here Fries was specifically allowing for a situation in which speakers regularly distinguished the pronunciation of two classes of words, but the phonetic distinction did not serve as a signal to the listener and therefore was not contrastive. In other words, Fries made exactly the opposite conclusion A. A. Hill did in the quotation given above when he was considering a similar situation.

A second consequence of Fries's focus on predicting the recognition responses of the listener was that the nature of what

constituted an explanation for him was quite different from the explanations used by linguists of the first tradition described earlier in this paper. Notions such as the elegance, simplicity and generality of the patterns described were subsidiary considerations to listener reactions. Fries's constant question was "How does the listener know that this is the intended interpretation?" In answering such a question Fries could not use deep structure trees and deleted elements, since, for his purpose, they are post-facto. They describe the meaning of the sentence after the sentence has been processed and understood. If sentences such as John is eager to please and John is easy to please differ in meaning, what is there in the speech signal which tells the listener that this is so. Tree diagrams showing that John is subject of please in one and object of please in the other merely describe the difference in meaning. To function in the communication of meanings from one individual to another the signals must be formal, observable and public features of the language or of the context in which the language occurs. Since people are not mind readers, communication cannot occur without such public features.

Fries's focus on the signals in the language to which the listener reacts had a third consequence as well. The notion signal implies two people, a sender and a receiver. Fries was never interested in the language system of one person, but rather in the language of communities of people. (See his discussion of Sledd's analysis given above.) The smallest possible community is two. Of course no two people have the exact same varieties of language,⁷ and the differences between the languages of each pair of individuals are unpredictable. But if Abe is to communicate a message to Bill, he must be able to predict within general limits the recognition responses Bill will make to the language he uses. Thus while individual differences are inevitable, it is what Abe and Bill have in common which enables the two to communicate. That is what Fries wished to describe. Because of his focus on communities of speakers, Fries did not refer to idiolects in his writing or use this concept in his work.

A fourth consequence of Fries's focus on predicting the recognition responses of the listener was his relative lack of interest in the grammaticality of strings. Since he was attempting to predict how listeners would respond to strings he did not need to focus on the degree to which these strings conformed to the grammatical rules of English. He needed merely to ask 'given that this string occurs in a context, how are native speakers of English likely to interpret it, and what features of the string are likely to cause them to interpret it in the way they do?' One can see this emphasis in the fact that Fries frequently used headlines (such as Marshall Proposes plan to help prostrate Europe) and telegrams as examples. (That is, he considered language which others might well call semigrammatical.) Further, one can see this in the fact that Fries did not discuss

grammaticality in his writings, and indeed gently chided those whose work focussed on distinguishing grammatical from ungrammatical sequences. In discussing the work of the generative transformational grammarians he said:

. . . a number of fundamental questions have been raised for which adequate answers do not seem to be available in the published materials. Valid criteria for the judgements of "grammaticality" as applied to sentences are essential for a "generative" grammar. The theoretical and practical principles upon which the criteria now used depend seem hard to find. (1963.91)

I suspect that in his view, these criteria were hard to find in transformational literature because they did not exist.⁸ Certainly no set of criteria he had were capable of satisfying him.

Now it must be admitted that certain steps in his work did imply noting ungrammatical sequences. Specifically, when he gathered and organized his data he used occurrence and non-occurrence in certain test frames to locate words and constructions which he wished to analyze. (See, for example the test frames he used in the Structure of English chapter five to locate words of the four major classes.) Certainly non-occurrence in a test frame implies an ungrammatical sequence. However, as I have already stated, he used test frames as a preliminary to his analysis, not as the analysis itself.

Chapter V of the Structure of English has been taken by many reviewers as stating my general criteria for "determining the parts of speech". This Chapter V, however, gives only a sketch of the procedure by which I procured from my corpus of material a large body of words to be examined in order to find out what the characteristic contrastive markers of each class were. (1960.150)

Remember that his goal was not to describe where certain words occurred, nor was it to explain why certain words did not occur in certain test frames. Instead, his goal was, given that a certain word was, say, a noun (or class one, in his terms), what told the listener that it was a noun. For Fries, working with an intuitive notion such as grammaticality as an initial technique of analysis was one thing. (Fries was never one for mechanical discovery procedures.⁹) However, to elevate grammaticality to a major theoretical concept and a goal of linguistic analysis was quite another.

We can see, then that Fries's model of language and of linguistics, with its emphasis on predicting listener reactions was essentially an experimental model. That Fries never engaged

in systematically controlled experiments resulted partly from his background, which was religious, literary, and humanistic, and partly from the nature of the materials he was studying. He knew without experimenting that speakers of English agree that Ship the sails tomorrow is a request, while The ship sails tomorrow is a statement, and Ship sails tomorrow is ambiguous. A carefully controlled experiment would only tell him what he already knew. Still, the possibility of experimental confirmation or disconfirmation is present within his model. The ultimate measure of the validity of the grammar was whether it could be used to predict the recognition responses of the listener.

By contrast, the patterns discovered by linguists working in the first tradition are self confirming. Once one discovers a pattern, its very existence lends significance to it. Perhaps that is one of the difficulties transformational grammar and its descendents are struggling with today. When is a pattern not significant? That is certainly a basic question for us all and one which must be answered. One way of discovering (or perhaps I should say rediscovering) such basic questions is the careful, systematic, sympathetic examination of the works of those who have gone before us. That is one of the uses of history.

Footnotes

¹ All italicized words and phrases in the quotations in this paper were italicized in the originals.

² The following quotations are good examples of later statements of goals by linguists in the first tradition:

The grammar, then, is a device that (in particular) specifies the infinite set of well-formed sentences and assigns to each of these one or more structural descriptions. (Chomsky 1964.9)

The grammar as a whole can thus be regarded as, ultimately, a device for pairing phonetically represented signals with semantic interpretations, this pairing being mediated through a system of abstract structure generated by the syntactic component. (Chomsky 1964.9-10)

The grammar is a system of rules and principles that determine the formal and semantic properties of sentences. (Chomsky 1975.28)

I assume that a grammar of a language is a system of rules that relates sounds in the language to their corresponding meanings, . . . (Lakoff 1971.232)

³ See also Harris 1944, 1946 and Hockett 1947 for more explicit uses of the criterion of simplicity.

⁴ See, for example, Halle 1962.

⁵ I do not mean to claim that extreme abstraction is a necessary result of the emphasis on regularity and simplicity. Indeed Harris's view of grammar and of transformations has always been much more concrete than Chomsky's or Lakoff's.

⁶ I do not intend to imply by this that Fries's basic goals changed significantly over the years. Rather it is quite clear that the differences in wording one encounters in his writings between 1927 and 1967 were largely a factor of making explicit in later statements what was implicit in his earlier statements.

⁷ In fact Fries believed that even individual monolingual speakers did not use completely regular self-consistent systems. (See Fries and Pike 1949)

⁸ In Fries's view Chomsky's attitude toward the language of real people using the language for their own purposes in their lives was dangerous. Fries often referred to Chomsky's statement in Texas in 1958 as an example of this attitude. Chomsky said:

The trouble with using a corpus is that some authors do not write the English language. Veblin, for example, speaks of "performing leisure", and the verb perform cannot take such an object. (Hill 1962:28)

Whatever the merits of this example, the philosophy behind it amounted in Fries's view to a return to the old nineteenth century prescriptivist attitudes. On the one hand it indicated to Fries a total disrespect for what is actually said and done by the speakers of the language, leaving the linguist only with his intuitions for what ought to be said. On the other hand Fries felt that this view implied that linguistic innovation in a language could only come about by breaking the rules and using something other than that language. If we agree that Veblin was breaking the rules of English and not using the rules of English, then what language was he using? Clearly it was not German, Spanish or some other language. Just as clearly, it was not nonsense.

Chomsky later returned to this issue and said that he had been misinterpreted. He did not mean to imply that such phrases as performing leisure were "being 'censored' or ruled out or that [their] use is being forbidden." (1961:239) Rather,

Given a grammatically deviant utterance, we attempt to impose an interpretation on it, exploiting whatever features of the grammatical structure

it preserves and whatever analogies we can construct with perfectly well-formed utterances. We do not, in this way, impose an interpretation on a perfectly grammatical utterance. (1961.234)

I suspect that Fries was not reassured by this statement. On the one hand, it established two means of understanding utterances produced by speakers; one by applying the rules of the language, and the other by applying analogies with these rules. Chomsky is quite vague in describing the differences between these two processes, however, I doubt that Fries would have wished to admit that the two were distinct. (Note that part of the difference between Chomsky and Fries results from their different goals. Chomsky was attempting to describe regularities in the system abstracted from its users and uses. As a result, departures from these regularities can best be dealt with as extensions of already existing rules. Fries, on the other hand was attempting to describe the signals which speakers and listeners used to interpret the various utterances they encountered. In this case, a signal is a signal, whether it occurs in a grammatical sentence or not.)

More important, I believe, in Fries's eyes than the establishing of two means of understanding utterances would have been the philosophical implications of Chomsky's position, for even though Chomsky had a different intent, his basic position was that of the nineteenth century prescriptivists who denied that actual usage was relevant to correctness in language and said that what was correct was correct because the rules allowed it. Compare this with the following statement published by Lyons shortly after Fries's death, in which Lyons describes the fundamental beliefs of transformational grammarians.

... whether a certain combination of words is or is not grammatical is a question that can only be answered by reference to a particular system of rules which either generates it (and thus defines it to be grammatical) or fails to generate it (and thereby defines it to be ungrammatical). (Lyons 1968.153)

⁹ However, Fries was interested in presenting his techniques of analysis in sufficient detail, so that others could see what he had done, and perhaps replicate his analyses should they wish to.

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APPENDIX

The following statement was prepared by C. C. Fries as a lecture handout in 1965. It presents a more extended description of his goals than could be included in the text of this paper, but presents his clearest, description of these goals.

Summary Statements of One Approach to Grammar

by Charles C. Fries, 1965

1. All language concerns itself with meanings. Or, perhaps, we should say rather that human beings are basically concerned with meanings and use language as their tool to grasp, to understand, and to share meanings. It is the linguist's business to turn the spotlight on the tool--language--itself, in order to examine the material of which it is composed, and to identify and understand the ways this material has been selected and shaped to accomplish its function of mediating meaning.
2. Of the various kinds of meanings mediated by language there is usually, in any particular language examined, a layer of grammatical meanings as well as a layer¹ of lexical meanings and a layer of social-cultural meanings. Grammatical meanings are not the same from language to language.
3. Some examples of "grammatical" meanings in English are the following.
 - (a) Of the following three sentences, the first is a question; in contrast with the second, a statement; in contrast with the third, a request or command.

Is every window in this room open
Every window in this room is open
Open every window in this room

(b) In the sentence Hunters sometimes kill bears the performers of the action are identified as "hunters", and the undergoers of the action are identified as "bears". But in the sentence Bears sometimes kill hunters the performers of the action are identified as "bears", and the undergoers of the action are identified as "hunters".

In the Modern English sentence God loves (a) cheerful giver and also in the Old English sentence with approximately equivalent lexical items, Gladne giefend lufað God, despite the difference in the order of the items, the performer is identified as "God" in both and the undergoer is identified as "giver".

(c) In the Modern English phrase in any other monastery's things the constituent "other" is identified as belonging with "monastery"; but in the Old English phrase with equivalent lexical items in ænium oþerum mynstres þingum the constituent "oþerum" is identified as belonging with þingum".

4. Grammatical meanings such as those indicated in 3. (a), (b), (c), in English attach to grammatical structures. This attachment of specific grammatical meanings to specific grammatical structures is "arbitrary" (a matter of convention) and must be learned. Grammatical structures are also "arbitrary" arrangements and must be learned. The characteristics that identify each grammatical structure of English must be learned, and the specific meaning that English attaches to each structure must also be learned.

5. The grammatical structures to which grammatical meanings are attached in English are identified (in the stream of speech, and in the succession of graphic shapes of writing) by patterns of arrangement of formally marked form-classes, by correlations of inflectional forms, and by function-words.

6. These identifying patterns constitute the signals of the grammatical structures to which grammatical meanings are attached.

7. Descriptive statements of these identifying patterns of grammatical structures in English can be made in formulas the symbols of which stand for the contrasting or functioning constituents of the patterns.

8. The aim of such descriptive statements (in formulas) is to specify the essential identifying contrastive features of the grammatical structures to which grammatical meanings are attached. These formulas of symbols thus represent the formal structural patterns for which it is possible to predict with a high degree of regularity the recognition and action responses that utterances produced in accord with these formulas will elicit from native speakers of the language (in the community in which the language is spoken). The formulas thus specify the formal signals of the grammatical meanings; that is, the signals that the speakers must learn to produce in speaking

the language, and the signals that the hearers must learn to respond to in understanding the language.

9. The validity or the correctness of the formulas can be measured by the predictability of the various kinds of response that utterances formed in accord with these formulas will regularly elicit in the linguistic community of the language.

10. The formulas that specify the identifying formal features of the grammatical structures to which grammatical meanings are attached can be regarded as basic "rules" for producing utterances with valid grammatical structures.

11. The utterances that can be produced in accord with each of such formulas will not be a limited number of isolated sentences but an indefinite number of utterances in the sequences of a discourse: that is, they differentiate between those utterances that can initiate a discourse and those that constitute the text of a discourse unit.

1. The word layer here is not to be equated with the word level as used in such phrases as "phonological level", "morphological level", "syntactic level".

LINGUISTIC CREATIVITY
AND THE
LANGUE/PAROLE DISTINCTION

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According to the well-known Saussurean distinction between synchronic and diachronic linguistics, synchrony is the branch of linguistics that concerns itself with the structure of language minus time, and diachrony with the structure of language over time. Many studies have pointed out that there are serious difficulties in maintaining this distinction (cf. Coseriu 1970). A fundamental defect of the dichotomy lies in its inadequate conceptualization of synchronic linguistics. Constant fluctuation in the use of language (Saussure 1959) implies time, and time is then, of necessity, a dimension of synchrony. In order to attempt to deal with this problem Saussure makes a further distinction within synchrony, i.e., between langue (the static system of relationships) and parole (the act of speaking). The object of study in Saussurean linguistics is the static system, although Saussure does require that the number of signs in the system be "numberless," thus implying some provision for a constant creation of new signs and blurring the distinction between langue on the one hand and parole on the other.

Hjelmslev (1963) also states that we must seek to identify the constancy which underlies the fluctuations and changes of speech. This constancy comprises the system on which all texts are constructed, and with the aid of which new texts are composed, including all conceivable texts in the same language. While the emphasis here is decidedly on the static system of relationships, glossematic theory clearly has some principle for constructing texts embedded within it, and thus implies some sort of productive process. Also, for Hjelmslev, an additional property of language is that it has an "unrestricted number of signs" and the potential to form new signs.

The capacity to form new signs is termed productivity by Hockett (1958:575, 1973) in his elaboration on the defining properties of language. Productivity is "the property that allows the speaker to say something that he has never said nor heard before, and be understood perfectly by his audience, without either speaker or audience being aware of the novelty." Thus productivity/creativity is, at least in part, a feature of the dynamic side of synchronic linguistics.

Chomsky's (1965) distinction between competence and perfor-

mance provides the basis to an apology for analysing exclusively the static linguistic system within synchronic linguistics. He further proposes that linguistic creativity can be adequately accounted for as residing in a recursive syntax; a syntax which, although intended to be static, is couched inextricably in the terms of a dynamic metaphor.

Algeo (1970) proposes the distinction between system-oriented grammars, that describe the static information a speaker has about his language, and process-oriented grammars, that account for the actual production and understanding of particular sentence tokens. He argues that a competence grammar ought to have a description that is not incompatible with what we do know about how humans produce and understand sentences.

Within the competence/performance distinction, Lockwood (1972) successfully recognizes a further division of performance into two different aspects. Ideal performance includes the necessary provisions for activating a static relational model in the encoding and decoding of information. Actual performance adds an account of the production of deviant speech forms, as well as of non-linguistic limitations in speech.

This sharpening of the previous distinctions allows one to consider the various process-motivated aspects of linguistic form, involving the creative properties of normal discourse (including the creation and understanding of slang, cf. (Eble 1980)), without necessarily embroiling oneself totally in the host of open questions within traditional pragmatics.

Reich (1968) goes even further and proposes the possibility that "the knowledge of grammar (i.e. competence) is not separate from a performance mechanism." He further suggests that language behavior below the sememic level should be describable purely in terms of network activation, as opposed to network modification, and states that "new knowledge results in addition to or changes in the network."

In a paper on "Linguistic Structures and the Production and Decoding of Discourse," Lamb (1966) posits an "infinite set of well-formed sememic networks," and refers to this set as the stage of "message creation." In speech such messages are encoded or expressed by means of the activation of linguistic networks. Still, the bulk of the research in stratificational theory has so far focussed more on the detailed contours of the system of relationships than on its activation in encoding. And even less attention has been paid to the dynamic properties of the conceptual network and its function in discourse.

The observations by Lamb and by Reich thus give rise to a further division within Lockwood's ideal performance: that is the distinction between the process of activating the sub-semantic linguistic network (without intentionally modifying it), and the process of modifying the conceptual network in the exchange of information. It is in this latter aspect of ideal performance that Makkai (1980) sees true linguistic creativity.

We thus reach a five-way distinction within the synchronic and diachronic study of language, which is summarized in figure 1.

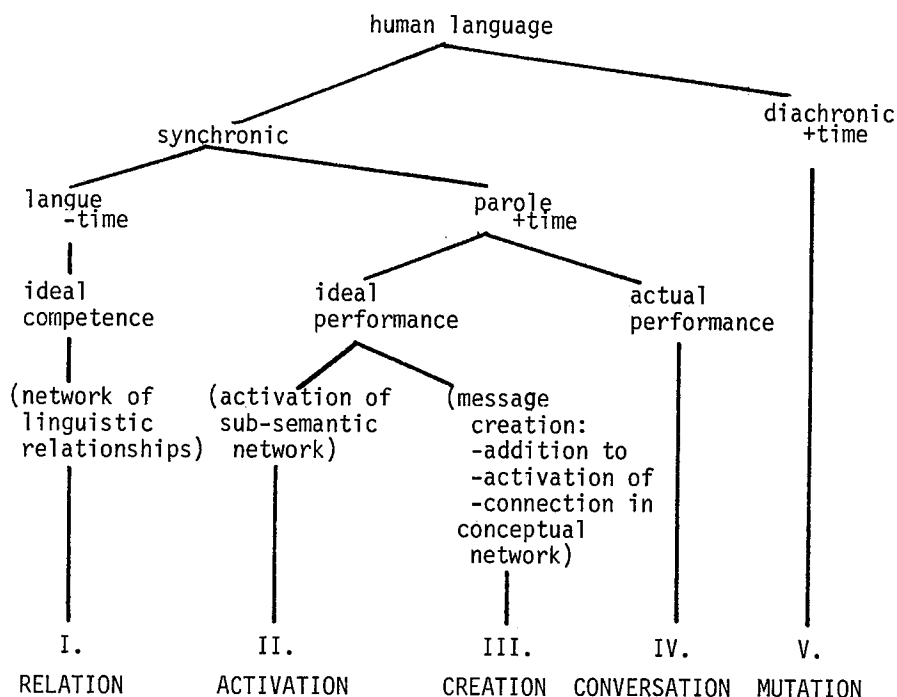


Figure 1.

Most modern theories of syntax allow for recursion, i.e., multiple traces through the syntactic patterns; some theories do so more explicitly than others. But it would be a mistake to restrict the notion of creativity to the output of a randomly generating static recursive syntax, because to do so would be to render it essentially trivial. The reason that such a view of creativity is insufficient is that it considers only Roman numeral I. (Relation) in Figure 1, with the occasional inclusion of Roman numeral II. (Activation).

What is needed is an integrated approach to discourse involving a semantic theory that includes I.(Relation), II.(Activation) and III.(Creation). It is often necessary to employ specific properties of III. in describing the structure of I. For example, no static theory of syntax has ever been successful in describing the conditions under which anaphoric substitution is possible. Such a static theory will predict the occurrence of utterances like sentence (1) at the opening of an ordinary spoken discourse with no extra-linguistic context:

(1) he did it in 49 B.C.

The reason for this failure is that although sentence (1) must be

accounted for in a description of I. as a possible trace, its privileges of occurrence in spoken discourse are conditioned by features in III., and they are constantly shifting as the discourse develops. For instance, the time span in which you can use the pronoun he is normally very short, since the speaker and the hearer must both be thinking about and be able to identify the individual referent intended by the speaker in order for it to be understood by the hearer.

It has been argued elsewhere (Copeland and Davis 1981) that the activation of or the addition to a conceptual network is a function of consciousness; that mention of a term in discourse causes it to become conscious within the hearer's conceptual network; and that consciousness is both temporally constrained and also bound by the number of terms that may simultaneously be in that state. A term that is conscious and identifiable (present in the system) in its domain and its particular is Given Gv. In English, a speaker typically signals his assumption that the conceptual nection for the intended referent is Gv by the use of a pronoun. Thus sentence (1) is perfectly appropriate if it follows closely on sentence (2) rather than the other way around. Additional discourse states (New, Nw: Computable, Cm: Some, Sm: and Recoverable, Rc) which play a role in the constantly changing context for linguistic encoding and decoding are discussed in Copeland and Davis 1981, as well as Topic, Tp and Focus, Fc. Other formal features of cohesion that also express discourse contextual information and require similar treatment to that of anaphora include article selection and ellipsis, not to mention "equi-NP deletion," "conjunction reduction," "gapping," etc. (cf. Halliday and Hasan, 1976).

Another major problem with the purely static approach is that the speaker/hearer's experience as well as his or her acquired knowledge is accomplished largely via tokens, i.e. in the realm of particular entities and events rather than of domains. These tokens consequently involve conceptual nections that are not present in the knowledge of the hearer prior to the exchange. And while a single trace through the syntax may be viewed as a token, it in the discourse-produced changes in the conceptual network that an utterance is most recognizably novel or unique. For example, (ignoring the normal figurative interpretation), the speaker of (2)

(2) Julius Caesar crossed the Rubicon

intends to impart information to his listener about a unique individual, a unique event and a unique river or stream. I refer you now to Figure 2. When uttering sentence (2), the speaker is instructing the listener to activate or make conscious the particular conceptual nections Julius Caesar and Rubicon which are already present in the listener's memory; to construct a particular nection CROSS, characterized by the superordinate domain nection CROSS, and add the appropriate propositional interconnections such that the information gets properly consolidated as a cognitive token and also gets related to identifiable conceptual domains, like ROMAN, COMMANDER, ... , CROSS, STREAM, etc..

The relational network notation employed in cognitive-stratificational grammar is a useful heuristic in the study of conceptual structure, since it enables us to represent the complex relations that obtain between nections in a clearly observable way. It also makes it possible for us to visualize the structure in such a way that the identifiable portion, i.e. that which is already present in the system, is easily distinguishable from the portion added through the decoding of a linguistic expression, i.e. the new knowledge. A partial representation of the network for sentence (2) is given in Figure 2., and for sentence (1) in Figure 3.

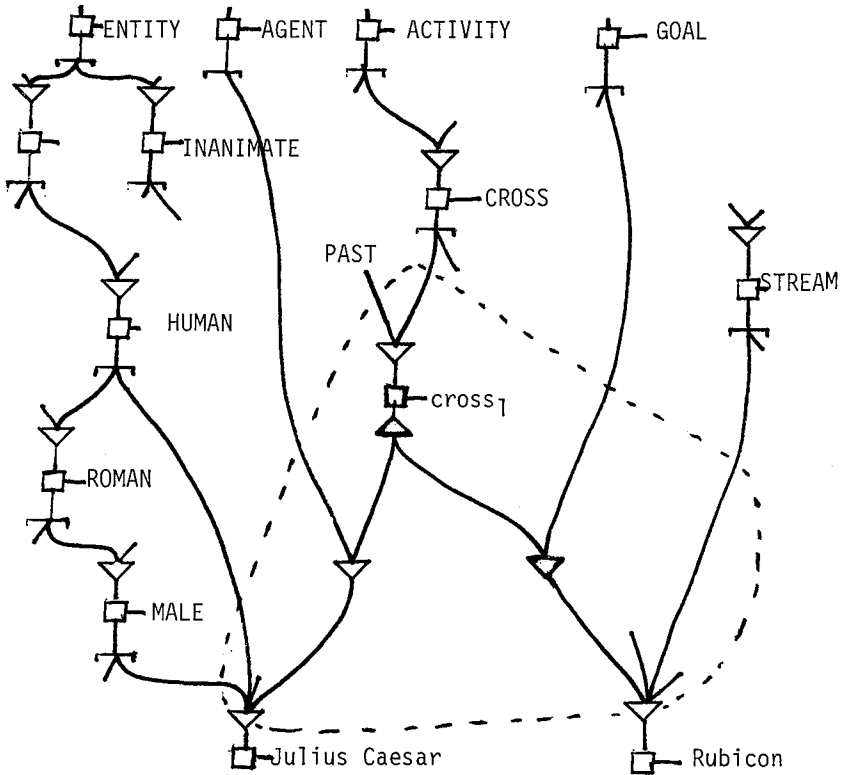


Figure 2.

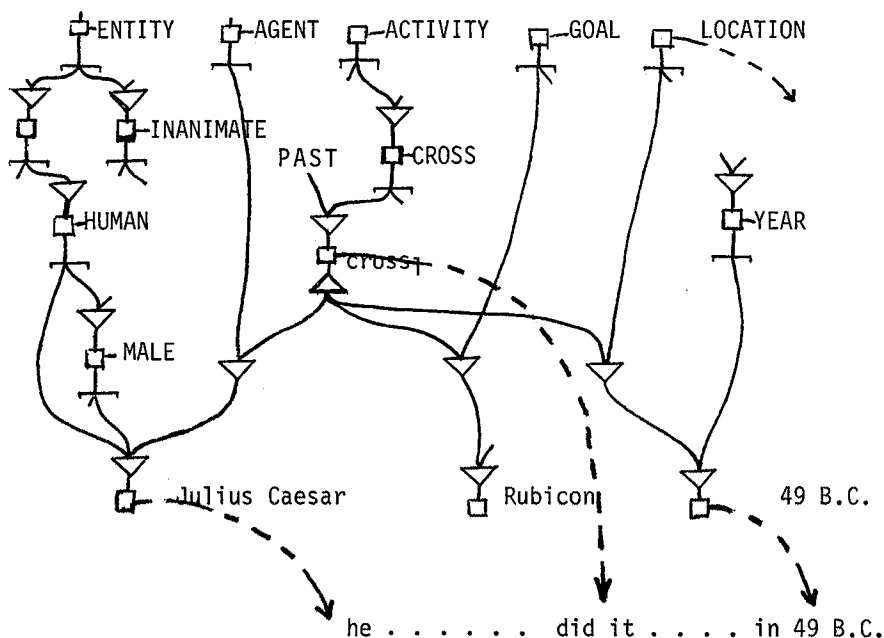


Figure 3.

Following Boost (1955) we may refer to the added portion in the conceptual network as the Rhema and the portion already present as the Thema. The Rhema is indicated in sentence (2) by the lines and nodes within the dotted circle in Figure 2. With the exception of truisms, every utterance represents a newly created Gestalt and must, then, be viewed as more than just a semantic trace.

It might be asked why it is necessary to posit a unique cognitive particular for the activity nection cross, in Figure 2. Let us assume that the speaker goes on to say sentence (3):

(3) but George Washington crossed the Delaware.

Here the speaker is invoking another particular instance of a crossing with its requisite crosser and crossee (see Figure 4). The discourse memory (cf. Davis and Copeland 1980) of this exchange will subsequently contain both token events cross₁ and cross₂, with unique propositional connections identifying which crosser did which crossing. Otherwise the memory would contain only the information that one or the other crossed one or the other. If we now add sentence (2) to sentence (1) in its conceptual representation we get the diagram in Figure 4.

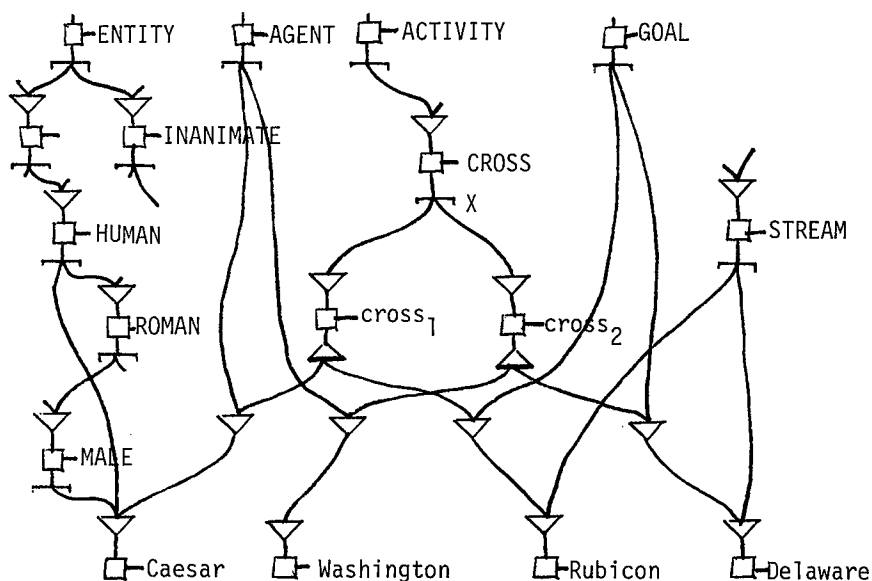


Figure 4.

Conceived in this way, the conceptual structure contains particular nections, having specific reference, at the bottom, i.e., nections having no extension. The upward connections express propositional relations stored in the semantic and episodic memory, and furnish the relevant thematic contexts for discourse. Such propositions include class/member relations, event/role relations, part/whole relations, etc. The conceptual structure also contains the mechanism for the creation of "numberless" new tokens of knowledge and thought. Thus, thought and language are seen as being very closely congruent, and they exhibit a similar creativity.

It now becomes clearer in what explicit sense we can conceptualize Lamb's "infinite set of sememic networks." In Figure 4., the or node under the ACTIVITY domain CROSS, labelled X, is an open set, i.e., the number of token crossings is conceptually limitless, as it is for all other domains. Our performance-competence enables us to create new nections and new interconnectivity at will. Thus, as in artistic metaphor, we bring into association conceptual nections (concepts) that were hitherto unconnected and predicate new things of old concepts (cf. Tyler 1978).

Hockett identifies the mechanism involved here as analogy. Indeed, human thought is largely metaphorical, and in thought processes metaphor is the rule rather than the exception. Because of the complexities in our cognitive interconnectivity we have the ability to experience fluently one kind of thing in terms of another. Thus, to give only one example, we may look at an ACTIVITY

as though it were an ENTITY, as in sentence (5):

(5) the crossing was difficult

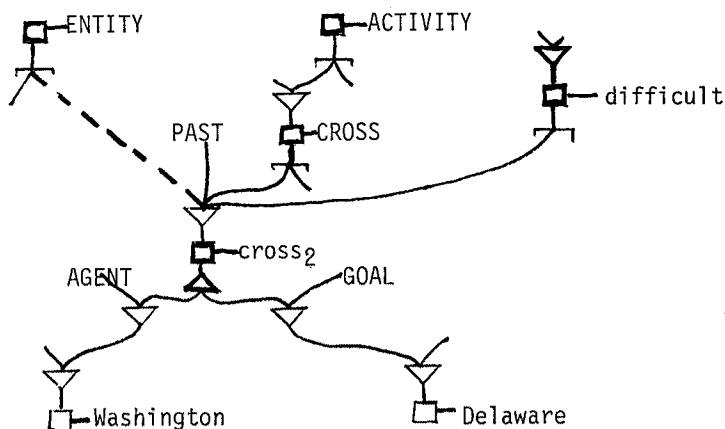


Figure 5.

In this way it is also possible to see language acquisition and language use in discourse as involving similar processes. New domains are created only when they are required; new particular nections are constantly being produced.

While it is clear that conceptual nections are not well-defined - think of Labov's cup - it is the case that speakers act as though they do know what they are talking about.

My purpose in this paper has been to attempt to evoke a context for analyzing linguistic creativity that is compatible with currently available relational theories of linguistic structure. The Saussurean distinction between langue and parole has resulted in an undue emphasis within linguistics on the structure of linguistic form, most notably on the structure of and inter-relationships among sentence types, rather than on the structure of linguistic discourse.

Although we have at times been given the choice in linguistics, it is not the case that we must choose between the study of language as a static substance and the study of language as a creative activity. The langue/parole distinction is not valid as a dichotomy; part of langue is parole, and part of parole is langue. The study of both systems is more successful when the study of one is coupled with the study of the other.

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FROM LINGUISTICS TO SEMIOLOGY

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The theory of the double articulation offers a pattern in reference to which semiological research can be most fruitfully pursued. It should not of course be considered by the semiologist as a model, since double articulation is precisely what characterizes languages in contradistinction to other communicative instruments. But what deserves imitation is the way a theoretically valid definition was arrived at, enunciating the features that are necessary and sufficient for an object to be called a language. That definition is based upon the functional analysis of data : a language is defined in consequence of its function as an instrument of communication. Linguistic units, in their turn, are defined in reference to their function : a significative function for the units of the first articulation, that of experience into its elements, the monemes ; a distinctive function for those of the second articulation, the phonemes. This last term points to the phonic, that is to say vocal character, of the channel used by languages, a feature included in the definition.

Also apparent in the definition is the Saussurian conception of the linguistic sign : monemes are two-faced units ; they have a 'semantic content' and a 'phonic expression'. They present an analysis of human experience that differs from one community to another. In other words, they show how the world is apprehended by man within a given culture. From one language to another, monemes and phonemes differ in nature as well as in their mutual relations. Consequently, they are to be considered 'conventional', which means 'not nature-given'. This reminds us of Saussure's 'arbitrary nature of the sign' and his stressing of 'the linearity of the signifier', a consequence of its auditory nature and vocal character. What is emphasized is the fact that speech is produced and perceived as a succession - of phonemes, words, sentences - according to a single dimension, that of time.

This fundamental linearity of the linguistic signifier is responsible for the manner experience is analyzed into the equivalents of monemes and for the necessity, for all languages, to secure a possibility of reconstructing the initial experience from the linear succession of monemes in the utterance. This is what we

call "syntax".

Reduced to key words, the definition could be summed up as follows : A language is an instrument of communication by means of conventional doubly articulated vocal signs. It is made of systems, i.e., sets of units that alternate in identical environments.

Note that what is so defined is a language (Fr. une langue), neither language in general (Fr. langage) nor language as an abstraction (Fr. la langue). Linguistics will be defined as 'the scientific study of language and languages'.

We shall now see how the criteria which are implied by double articulation should be either retained or dropped in order to delimitate the domain of semiology as against linguistics.

We stipulate at first that the object of semiology is the study of conventional systems by means of which men communicate, with the exception of languages whose study is a special discipline : linguistics.

The criteria we retain as defining the domain of semiology are : communication by means of signs, convention, system. Note that we leave out the specifically linguistic ones : double articulation, vocal nature of the signifier.

We shall begin with the examination of the most elementary and best identified systems whose main function is, without any doubt, communication, and where communication is achieved by means of conventional signs.

The operation by which data are confronted and units and classes of units are established is, just as in linguistics, commutation. It requires the coexistence of semantic content and perceptible form. Units are not identified solely on the basis of their formal identity, but on that of their function, i.e., the part they play and the relations they entertain.

In order to establish communication with another person, one can address him. Saussure called this an act of speech. One can also make a hand gesture or a nod in agreement or a shake for denial, etc. Those behavior, we call semic acts and an act of speech is just a particular type of semic act. The words, or the gestures that carry the messages are signals, whether they be behaviors or artefacts. The signal, together with the message it carries constitute what I shall, after Buyssens (1943), call a seme. The seme will be the framework for semiological analysis.

A seme =

message
signal

It should be noted that, from a Saussurian standpoint, a seme is a sign. Although he felt the need for it, as it appears throughout the Course, he had never clearly isolated the minimal sign. The term seme made it possible to reserve 'sign' for minimal signs. The basic distinction is the one between, on the one hand, semes and signs which are two-faced entities, and, on the other hand, figures, parallel to phonemes, which have no significant face, no significative value. Figures are not signifiers but the constituents of signifiers. Some systems operate with figures, others do not. The perceptible face of the sign is not necessarily analyzable into figures.

*
* *

The sketchy typology of semiological systems which follows is essentially based upon the degree and nature of the articulation of their semes.

Four types of semes can be isolated :

In a first type, commutation is performed between unanalysed semes : a global signal is associated with a global message : they both commute as a whole. An illustration of this type is offered by a system of two semes : the white sail and the black sail in the legend of Theseus on his way to fight against the Minotaur ; on returning from the expedition, according to a convention established between Theseus and his father, the outcome of the operation was to be indicated as follows : the message 'success, Theseus alive' would be indicated by the signal /white sail/, the message 'failure, Theseus dead' by the signal /black sail/.

Type I	'failure'	} One seme One sign
	/black/	

Belong to that type also, traffic lights (a system of three semes) : "/red/ ~ 'Cars wait' ", "/green/ ~ 'Cars proceed' ", "/amber/ ~ 'cars ... prepare to stop' ... or 'hurry up !' " if we derive the meaning from the behavior of drivers.

In this type we can include systems consisting, as it were, of only one seme like the white stick of the blind which only commutes with its absence.

In a second type, commutation makes it possible to divide semes into units of the moneme type, endowed with both form and meaning : such units are signs. In a triangular roadsign with the profile of a cyclist, two signs can be isolated : 1. " 'danger' ~ /triangle with

red edging/ ", and 2. " 'cyclist' ~ /profile of a cyclist/".

Type II	'danger'	'cyclist'	} One seme Two signs
			

" 'Danger' ~ /triangle .../" commutes, in the context "cyclist", with a sign " 'interdiction' ~ /red crown/ " on the roadsign for "cycling forbidden", and with a sign " 'meant for' ~ /blue crown/ " on the roadsign indicating "lanes for cyclists". The sign with a signified 'cyclist' and /profile of a cyclist/ as a signifier is extracted by comparison with other roadsigns where other profiles, such as /cattle/ or /stones falling/ appear in a red-edged triangle. The analysis of the signifiers cannot be carried any further. Roadsigns form a system of 95 semes.

In a third type, we find global messages that commute as wholes with one another, but the signals can be analyzed into units of the phoneme type, which we call figures : they make it possible to keep semes distinct but they have no meaning by themselves. To this type belong distinctive numbers (as opposed to quantitative numbers ; the French distinguish between numéro and nombre, the Germans between Nummer and Zahl), in other words, sequences of digits with no significant values such as telephone or social security numbers. The subscriber allotted the telephone number 702 28 15 is someone different from subscriber number 702 29 15 or subscriber number 702 82 15 ; he is not superior or inferior to any of them. To this type belong also military bugle calls, radio or television

Type III	'put out the lights'	} One seme A definite number of figures
		

program signals in which the properly musical value of a melody is not at issue : this sequence of notes is made different from that one, so as to produce different signals carrying different messages : news, weather report, games, etc. With a limited inventory of digits, all the numbers of the telephone subscribers can be formed, with a limited number of notes, all the military bugle calls, all the radio or T.V. program signals one can dream of can be worked out. Such systems may offer

thousands, nay, millions of semes.

The fourth and last type is the one represented by double articulation, which is the specific mark of languages.

Type IV	'John'	'is'	'ready'	One seme Three signs Nine figures
	dʒ	ɪ	n i z r e d i	

It has been pointed out that there are doubly articulated systems other than languages : social security numbers may be analyzed into significant units: for the French, "sex", "year (of birth)", "month (of birth)", "department of birth", "place of birth", "rank on the monthly birth record" : e.g., "1.08.04.73.221.002". But this first articulation, which plays a role when ascribing a number, has no part in the interchange between the insured person and the social security administration. Normally, the person insured is not aware of the possibility of analyzing his number. Besides, whatever first articulation is to be found is a reproduction of an initially linguistic articulation.

Type III : for the insured person

'Jean Durand'												One seme One sign Thirteen figures
1	0	8	0	4	7	3	2	2	1	0	0	2

Type IV : for the administration

'sex'	'year'	'month'	'dep't'	'town'	'rank'	One seme Six signs Thirteen figures
1	0 8	0 4	7 3	2 2 1	0 0 2	

This last example points to the fact that what matters in communication is the seme as a whole, irrespective of the type of analysis the various parties involved are operating with. The articulation arrived at by the semiologist (or the linguist) is not invalidated by the fact that it is not made conscious use of by a particular party.

On the basis of articulation, four types of *semes*, and only four types, have been isolated and documented. But, of course, we have to reckon with the possibility that a complex of semiological systems may offer different types of articulated *semes*. We may come across situations that will remind us of what happens in languages where besides type four, we find systems of interjections which do not partake in the first articulation of language, but may be analyzable into phonemes and, consequently, belong to our type three.

The units isolated on the basis of articulation can be grouped into classes and the next step in our study is the examination of the relations, compatibilities and hierarchies between the classes thus established. We can devise grammars, that of the road signs, for example, but it should be kept in mind that syntax proper implies an articulation into signs (two-face units of types II and IV). Relations in a system of type I can only be of a paradigmatic nature.

Distinctive features are to be found in all four types : they work on the plane of the signifier, on the level of the minimal unit : white or black, red or green or amber in type I, triangle or crown, blue or red in type II, phonematically relevant features in type IV.

What precedes is, by no means, the only possible typology. One based upon the varying nature of the relation between the signifier and the signified is available, but its presentation would exceed by far the scope of this paper.

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WHITEHEAD AND LAMB: A NEW NETWORK OF CONNECTION

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Abstract

Independently, scholars of different times, disciplines and academic paternity **basically develop similar, sometimes identical, concepts.** The discovery of such interconnections across the boundaries of intellectual histories has itself an academic value justifying further examination. However, in the case of the connection between A. N. Whitehead and S. M. Lamb proposed in this paper, the value is more tangible. Whitehead's "process" concept has provided the foundation for a philosophical and a theological school of thought. In Lamb's independent work this is put into tangible form in computer innovation as well as linguistic theory. Another practical aspect is related to learning theory and pedagogy.

i. Hypothesis: proposed a unique, previously unarticulated, connection between "network theory" and the process, Whitehead philosophy.

ii. Overview of Whitehead and Lamb Connection

Alfred North Whitehead, outlining his philosophy of organism, asserts that "life is essentially periodic," and proposes a form of the Hegelian triad as a model for reality. Whitehead uses the term "prehension" to signify the organic wholeness of experience. For him the apprehended and prehended meaning are indispensable parts of the whole. In formal education he argues, as he puts it, for the banishing of the unrhythmic collection of distracting scraps. He advocates instead the "weaving of a harmony of patterns" in which the development of mentality exhibits itself as a rhythm involving an interweaving of cycles. He warns of "inert ideas" and "the fallacy of misplaced concreteness," the taking of one aspect of a part and treating it as the whole. The Whiteheadian phrasing itself is frequently similar to that of Lamb's in his method and study of the linguistic dimension of the semiotic potential. Lamb's describing the interlock of language and culture has put Whitehead's philosophic concept into a concrete, useable form.

In Whitehead's elaboration on these themes the overlap becomes even more apparent. He proposes that the universe is like an organism, a dynamic and creative whole characterized by

integration, not separateness. As he says, "Nature is a structure of evolving process, and the universe of knowing and reality is a process in which its parts are interwoven." (Science and the Modern World. New York: The Macmillan Co., 1925, p. 106.) He advocates acceptance of the notion of the interdependence of things and postulates that "everything is sensitive to everything else." (H. H. Titus, Living Issues in Philosophy. American Book Co., 1953, p. 100.)

Whitehead's work has generated a school of philosophy, a theological system and educational theory. In the connection between his "process" and Lamb's "network" concepts, students of linguistics and philosophy can observe a philosophy made flesh such that applications and validations are available to members of both schools.

iii. Origin of Hypothesis

During a discussion with two Whiteheadian philosophers, explicating theories of linguistics including the comparative advantage of anthropological linguistics--principally the work of Lamb and Halliday, the writer proposed an hypothesis that there was a valuable, unexplored interlock of assumptions and principles between Whitehead's process and Lamb's network concepts. The philosophers' interest persuaded the writer to initiate a symposium. As a result, in the spring of 1982, five representatives of the two schools were brought together to examine the hypothesis. S.M. Lamb (Rice University) and John Regan (Claremont Graduate School) spoke to the anthropological linguistic, distinguished philosophers and theologians, John Cobb, Director, David Griffin, Assistant Director, of the Center of Process Studies, and Arbinda Basu (Auro Bindo International Center, Ashram Pondicherry) represented the Whiteheadian process position.

The following section contains statements from that discussion as examples of the evidence that led to the acceptance of the hypothesis.

iv. Report of the Examination

S. M. Lamb presented his position using two principal examples of the nonstatic relationship character of meanings. Regan introduced the hypothesis explicating some applications and examples of the overlapping ideas. Cobb, Griffin and Basu spoke to the theme of language and broader human experience. Basu expanded the Lamb/Whitehead process/relationship concepts to include Hindu thought.

The following are examples of their statements.

Regan: "The network model provides a means for describing the lines along which communication meanings are built, not of static isolates but processes, perhaps a slightly different meaning of that Whiteheadian philosophers use but not much. Lamb's theory begins with the assumption that cultural and linguistic information is a network of relationships, an integrated whole. That

sort of approach may seem to lead to mind mapping. And it did with valuable results.

At a dinner party some time ago, I asked the president of a prestigious science college what in his field of inquiry he would most like to know. His response seems all the more remarkable because we had never uttered a philosophical word to each other. To boot, the guests were, in the main, financial consultants and business people, such that the focus of the conversation, if not hardcore materialism, had been down-to-earth reality, certainly none of the other worldly thoughts of anthropological philosophers. What this scientist wanted to know was how all things are interconnected. "Interconnected?" I asked, puzzled.

"All things are--you and me, this chair and you, this cup and me, this cup, this chair, this house--connected by threads," he replied. 'By threads,' I inserted, thinking of tripping and other such unserious ideas. Probably recognizing a layman in his listener, he turned for support to an engineering professor beside him. He elaborated, 'By measurable threads.'

I told him about the interactive, relationship, network concepts of the anthropological linguistic school. A year later I had a chance to ask this president whether his field had made any progress on that issue. It had, he replied, but then we were interrupted by the soup. Hence that is all I can report on the opinion. I can be more informative about the interconnection my colleague, Sydney M. Lamb, sees within the human linguistic and cultural information systems. The types of threads that Lamb examines may be of a different sort than those of the physical scientist, but they are as real and have as tangible an outcome.

To get a sense of the network approach, consider what would be necessary if we listed what a child knows about the word "dog." His information about that word staggers the imagination--its phonology, its syntactic capabilities, the vast information about the word itself; for example that dogs go with cats, that it can rain cats and dogs, that the word can be used as an insult, that dogs bite, have tails, fleas, scratch, go for walks, that mailmen have special relations with them, that there are jokes and puns, hot ones and sore ones. That universe of interconnection with all the verbal and nonverbal information brought with that word is actually its meaning (apart from all the individual and personal memories attached). Without including that interconnection, a computer program could not approximate human information. It does, of course, mean that language, as Lamb says, is messy, that it is a process of moving, overlapping meaning.

When Lamb began to depict the types of relationships that exist between words and all the linkages, there was no code. How do you describe this mass of meaning? Are all the parts, the attachments equal? Is that one between "hot dogs", "cats and dogs" or "he's a dog," "it's a dog of a day" equal? There are other problems. A different sort of coding method was necessary to describe these different connections. The development of standardized means of noting them was not required for the philosopher Whitehead but it was for others' work.

Lamb: Linguistics is bringing us back from the fragmentation of knowledge that has been characteristic of universities during the last 100 years or more into an integration of all the fields of knowledge with the notion of networks of relationships as the unifying principle.... New York City of last week was not the same thing as New York City today. Many people have left and gone other places, new people have come in. Everybody is a little bit older, food has come in, and so on.... What we think of as our direct sensory experience of the world has actually been filtered through our conceptual and perceptual systems, which are networks of relations, and it is that processing that goes on within those cognitive networks (that is, our minds) that is the only direct experience. To a large extent what we naively think of as our knowledge of the world gained through perception actually turns out to be that is out of correspondence with reality, because the filtering process of our conceptual systems has distorted the data as it filters through.

Cobb: What you have said fits process thought. We are especially influenced by Alfred North Whitehead who is not unrelated to Pierce. Even though we come out of quite different traditions there is some connection. Our focus has not been upon language but rather upon the world, so to speak. Whitehead says very similar things about the nature of the world that you were saying about the nature of language.... If you begin with my seeing the cup, then the view is that there is the cup, which is the enduring object out there which could again be analyzed into smaller enduring objects which would have still that same kind of a substantial character, and then there is the "I" who sees the cup. Modern philosophy began with the famous "cogito" of Descartes. "I think therefore I am."... There is some readiness to consider other basic ways of thinking about what the world is made up. In the case of relativity theory, obviously, the very word points to relations.... For the Newtonians, there was an absolute space-time within which there were absolute locations. In relativity theory, that is all gone. So if there are no absolute locations and the entities which are in relationship are themselves not substances or atoms then what are they? In Whitehead's analysis we no longer mean something that endures through time, remaining self-identical with itself through time, but an event or an occurrence. He uses the word "occasion." So the world is composed of happenings rather than of enduring entities. And what each happening is is a unification of relationships to everything in its world.

Basu: Mind is not degrees of consciousness. It is something more fundamental. To take a special imagery, it is above mind, so to speak. But it is sometimes below mind, because it is absolutely vertical and horizontal at the same time. On the human level there is one theory that language starts with sensation. But the sensation itself is on a separate level and intuition comes down on the human level. So there is a connecting link among all of these. Nothing is absolutely disconnected. But for our understanding we have to make these levels.

Griffin: Your first statement, John Regan, about how all things are interconnected--that was Whitehead's fundamental goal with his own philosophy. He used the term "solidarity"--how do we understand the solidarity of the world? In Whitehead's view, rather than that you have a series of occasions of experience, and each one of those is a process. The essential meaning of the term "process" is of self-creation that is really just a synthesis of relationships. The technical term for relationship is "prehension" or feeling, so you feel or grasp orprehend the past, pull it into yourself and synthesize it. And because the actual things are processes, rather than substances which are already what they are before they enter into relations, the relationships are fundamental. This is so because you do not first have something that then is going to relate to something else. It is its synthesis of relationships. That was Whitehead's basic way of understanding the interconnectedness of the world.... Whitehead would say that the whole of the actual world is made up of nothing but these processes of creative synthesis. You have got a hierarchy of occasions of experience which are all same in kind. They are different in degree. The difference would be the degree of self-determination they have, the ability to make a novel synthesis of the relationships. When you have a psyche, a cell, a molecule, an atom, and an electron, you could talk about interaction at all of these levels.

As you, Mr. Lamb, structure it, you are interested in the mind and how it interacts with the brain, with the rest of the body, and how meanings are structured within those levels.... If all things are experiences, that means in principle you can talk about all things being influenced by meaning and all things expressing meaning. And in fact, Whitehead uses that language all the way down. He talks about "propositions." A proposition is a meaning. A proposition is something that exists--it is not the same as a verbal statement. A verbal statement is a particular articulation of a meaning. But a proposition would be the meaning that you could express in various different verbal statements. ... When you get down to the inorganic level, Whitehead tends to shift to another term called "physical purposes." But they are not different in kind from propositions. Hence you could extend the term proposition or meaning all the way down because you have the system of pan-experientialism, as opposed to a dualistic view which has dominated our views in the West....

We live in a world of concepts rather than of things. This does relate to something very vital in Whitehead's thought. He believes that we have two basic modes of perceiving, one he calls "perception mode of causal efficacy." And then another mode of "presentational immediacy." Those concepts tend to make us think in terms of enduring substantial things that will stay the same. But as Lamb points out, reality is changing every second. There is evidence that we do not live totally in a world of concepts but also in the world of flux even though it's sometime not as apparent to us. A Whiteheadian view would suggest we live in both....

One way to go on this...which was Whitehead's way was to try to think of objects no longer in terms of visual categories

or tangible categories but to think of them in analogy with our own experience. There is some truth to the notion that what we call experience is not spatial in the sense that a billiard ball is spatial. So, again, I think if we talked further we would see we were not in disagreement about not wanting to use visual categories to try to understand what a molecule is."

"That old model just has to go. It is gone already in physics; it has to go everywhere else too. Whitehead's central point here is that this is not a temporal process in the sense that this occurs and then this occurs and that is a firm foundation for this occurring; but rather this final event here, the final part of the process, which he calls the satisfaction--the total integration of the whole process--affects it from the very beginning. That is, it is going to be influenced by the very way that you received the data from the world in the first place. . . . On the one hand, everything is conditioned by the whole process, of objectivity there of pure intuition of the world."

"A different kind of hierarchy in Whitehead that may be the one we should have talked about in the beginning. We talked about a moment of experience. To some extent we can talk about there being different phases of a moment of experience, and this gives a kind of hierarchy. This connects somewhat with some of the things that Basu was saying too in this question about the objectivity of the world, intuition, and the object." . . . To such statements as the above during the three hours of dialogue, Lamb replied: "I'm very happy to know that I have been a Whiteheadian without knowing it."

v. Application

The child knows the "what is" and "what is not" of an incredible amount, in a fabric of interconnected verbal and non-verbal cultural information. This knowledge consists of a network, not of isolate inert details but dynamic processes. Of course in a sense, all science, each discipline, accepts that there is an organic wholeness to knowledge and, indeed, life itself, but (as one considers some of the attitudes, contents, and methods of education) the necessity that other views exert their effects is apparent.

Whitehead wrote that the lack of attention to the rhythmic character of mental growth was a main source of futility in education as is also unrhythmic collections of distracted scraps rather than "the weaving of a harmony of patterns." So too it is in the presentation of a part of the linguistic system as if it were separate unto itself rather than a node in a network. While Lamb has not specifically examined learning and curricular theory in his writings, the implication of his and Halliday's work is that insofar as meaning is a network of relationships and is defined by the wholeness of its content, the epistemological reality which a learner in school should encounter is broader than that traditionally experienced.

A characteristic of the anthropological linguistic perspective is the assumption of the coherence of the human linguistic system and the process by which it is learned. The best metaphor for this is in both anthropological linguistic interconnectedness of knowledge and Whiteheadian process theory. This itself could become an additional dimension of study for students of the linguistic and philosophic school.

The interrelationship of the cultural or informational system is such that entering at any thread of the weave will take the inquirer through the entire fabric. The child's information system is itself a process-like experience in which insights and breakthroughs occur in logical, but not necessarily linear, ways; hence a child's linguistic development is best studied as a complex of use, not simply as sounds, words, phrases, sentences strung together as parts out of the cutting box of the mind.

Consider the following statement from J. M. Barrie's Peter Pan as an example:

I don't know whether you have ever seen a map of a person's mind. Doctors sometimes draw maps of other parts of you, and your own map can become intensely interesting, but catch them trying to draw a map of a child's mind, which is not only confused, but keeps going round all the time. There are zigzag line on it, just like your temperature on a card, and these are probably roads in the island; for the Neverlands is always more or less an island, with astonishing splashes of colour here and there, and coral reefs and rakish-looking craft in the offing, and savages and lonely lairs, and gnomes who are mostly tailors, and caves through which a river runs, and princes with six elder brothers, and a hut fast going to decay, and one very small old lady with a hooked nose. It would be an easy map if that were all; but there is also first day at school, religion, fathers, the round pond, needlework, murders, hangings, verbs that take the native, chocolate-pudding day, getting into braces, say nonety-nine threepence for pulling out your tooth yourself, and so on and either these are part of the island or they are another map showing through, and it is all rather confusing, especially as nothing will stand still.

Of course the Neverlands vary a good deal. John's, for instance had a lagoon with flamingoes flying over it in which John was shooting, while Michael, who was very small, had a flamingo with lagoons flying over it. John lived in a boat turned upside down on the sands, Michael in a wigwam, Wendy in a house of leaves deftly sewn together. John had no friends, Michael had friends at night, Wendy had a pet wolf forsaken by its parents; but on the whole the Neverland had a family resemblance, and if they stood still in a row you could say of them that they have each other's nose, and so forth. (J. M. Barrie, Peter Pan. Puffin Books, 1968.)

He too understood the in-process multicontext of human experience, knowing, and learning. The sudden mentioning of "islands" and then "neverlands," out of sequence, suggests not lack of organization but rather much organization skill by the writer that can manipulate traditional sequences as a device for capturing, in his form and content, nonlinear features of the world. Barrie reintroduces points in accumulating details only after which the logic of the whole is apparent.

There is indeed a simultaneity about learning that defies linear interpretations. When a child learns his first words, he learns much more, for he is immediately precipitated into a world view, an affiliating system and a mode of thought. Like the mind, words, other linguistic units, are islands of clusters, where meanings show through each other. Each is a connected event that is caught up in a simultaneity. That fact once recognized provides more accurate viewpoints of learner and what is to be learned.

Communication media, their functions, learning and experience are complex wholes. Not only will they not stand still, but also their content like other "islands" is overlapped such that their combined reality shows through each other. Rather than being made of discrete parts lined up like blocks, they are a dynamic, moving process event. This characteristic instructs our understanding of children's communicative development, makes it less simple, but more like its reality. Systemic grammar had made this view a scientific, hardware fact. Whiteheadians need to know about it also.

The organic process concepts have become the foundation of an educative theory based on the rejection of a linear accumulation of knowledge and the acceptance of the learner and the learned as consisting of and producing a network of relationships.

Alfred North Whitehead's philosophy is an excellent marriage of the anthropologist's interest in form and function; the social semiotic and the theory of cultural relationships. It is a perspective that has fundamentally important applications to those applying the understanding of human communication to learning and the mind. The traditional view of children's learning is that it is a 1 = 1 event; however, the analogy is more that of co-occurrence of the learning. J. M. Barrie put it well in a paragraph, Whitehead stated that in a philosophy, and Lamb in a linguistic theory. Information is received and transmitted not only in a Morse code type of way, one signal after another, but also in a constellation formation. That context, rich in clues, defines its message such that there is, as Barrie demonstrated, a multiplicity of hints as to what is coming down the line.

vi. Conclusion

The conclusion of the examination of the hypothesis was that a hitherto unrecognized, noncausal connection could be demonstrated between the work of two epistemological thinkers,

A. N. Whitehead and S. M. Lamb, and that there existed sufficient evidence that further examination of the points, principles and applications should occur. The purpose of this paper has been to report the hypothesis, its examination and conclusion in order to provide the foundation for this examination.

The hypothesis of fundamental likeness of these schools has more than academic interest insofar as its acceptance could elucidate some important issues concerning each discipline and their application to the study of learning, the child's mind, and formal education. For example, views of linear-programmic learning, derived as Whorf and McLuhan proposed from habitation with modes of communication, influence behavior. Metaphors of the child's mind and knowledge other than linear should be brought forward to influence educational decisions and to contrast with the presentation of information of galaxial wholeness, (for example a Shakespearian play) as if it were a linear event.

The Barrie piece has served as a linkage statement among the two theories and their potential applications. The piece is itself a description of the process, interactive characteristics of the child's mind. As well the writing exhibits the characteristics of nonlinear creative sequence and interconnectiveness which receives short shrift from school learning practices that begin at the "front," the beginning of all learning as if it were the acquiring of a linear object.

Whitehead's expansion of the Hegelian triad of thesis, anti-thesis, synthesis is brought to a practical concreteness in Lamb's and Halliday's work as ultimately demonstrated in their computer application. It provides linguistic theory with one more linkage. In turn, this connection adds substance to Whitehead's theory and provides a conduit for the examination of other epistemological applications.

DIFFERENTIATION AND FREQUENCY OF LINGUISTIC ELEMENTS

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Nearly twenty years ago, I formulated a law concerning the relationship between differentiation and frequency of linguistic elements.¹ As far as I know, this law did not arouse any interest; nevertheless, I am still persuaded that his law is true, and therefore I would like to speak about it once more. The law in question is very simple and reads like this: the more frequently used linguistic elements are more differentiated than those which are less often used. By linguistic elements I mean letters, phonemes, words, groups of words. I shall give some examples illustrating this law and referring to spelling, phonetics, word formation, inflection, syntax, and vocabulary.

Spelling: The small letters are more often used than the capital letters, and, if there is a difference between small letters and capital letters, the former are more differentiated than the latter. E.g., in Greek, there are two signs for the small sigma (σ , ς) but only one for the capital sigma (Σ). In German, there is a difference between ss and ß; e.g. in Gasse and Straße, but there is no difference while using the capital letters: GASSE and STRASSE. In Russian there are two ways of writing the small d (д , д), while the capital D can be written only in one way. The same applies to the Russian ѣ (м or ѣ , but only ѣ). In different European languages, there are two ways of writing the small r, while there is only one possibility of noting the capital R. In Italian, there is a difference between apostrophe and accent in de' and tè, which may disappear while using capital letters: DE' and TE'. In French, the capital letters can be used without diacritic signs, so that E may correspond to five small letters: e, é, è, ê, ë. The Arab ciphers are used more often than the Roman, and this explains why in Spanish there is a difference between 5 = cinco and 5.^o = quinto, while V means both 'cinco' and 'quinto'.

Phonetics: The voiceless consonants are more often used than the voiced and if the number of voiced consonants is not equal to that of the voiceless, the number of the latter is as a rule larger. E.g., in English there is a voiceless [h], but a corresponding voiced consonant does not exist. In Indo-European languages, dentals are more often used than labials or velars, and this explains why Spanish d has three allophones (as in duro, padre, cantado), whereas b and g have only two variants (as in bueno, haber and gato, rogar). Oral vowels are more frequently used than nasal vowels, and therefore oral vowels are more differentiated, in other words they are more numerous than nasal vowels (e.g., in French).

Word Formation: There are more derivatives and compound words

from a frequent word like mother than from a rare word like olive. The meaning of the derivatives and compound words is less differentiated than that of the simple words, cf. Spanish claro meaning 'clear' and 'famous', while preclaro means only 'very famous'. In many languages, quite a different word serves to form a feminine in the case of most frequent words, e.g., son - daughter; a suffix is used with less frequent nouns, e.g., actor - actress; while the feminine is not formed at all from words which are seldom used, such as witness. The same applies to animal names. In the case of the most frequent animal names, there are two different words, e.g., bull and cow. With less frequent words, the difference is smaller, cf. lion - lioness. Finally, the feminine is not formed at all from words which are seldom used like hyena.

Inflection: In Latin, from the point of view of frequency, the indicative takes the first place, the subjunctive second, and the imperative last; this is why the indicative has six tenses, the subjunctive four, and the imperative only two. The present is more often used than other tenses, and this explains why in the present of the verb speak there are two forms: speak, speaks, but only one in the preterite: spoke. From the point of view of frequency, the order of the numbers is as follows: singular, plural, dual. This explains why in Old Church Slavic the singular has seven different case forms, the plural only six (there is no special form for the vocative), while the dual shows only three forms: one for the nom.-acc.-voc., one for the gen.-loc. and one for the dat.-instr. Among the persons, the third is most frequently used. Therefore, in the singular of the Russian preterite, there are three forms in the third person (for the masculine, the feminine, and the neuter), whereas in the other persons of the singular there are only two forms. The passive voice is less frequently used than the active, and this explains why in Latin the synthetical forms of the passive are less numerous than those of the active. In all Indo-European languages, the verb meaning 'to be', which on frequency lists occupies the first position, has the most complicated conjugation.

According to a frequency dictionary, the distribution of English strong and irregular verbs is as follows:

First thousand	85
Second thousand	28
Third thousand	15
Fourth thousand	14
Fifth thousand	11
Sixth thousand	3

To put it differently, the more a verb is used, the more it is probable that its inflection will be complicated. The same applies to French, where the distribution of irregular verbs is similar:

First thousand	74
Second thousand	40
Third thousand	21
Fourth thousand	22
Fifth thousand	13
Sixth thousand	11

As surnames are less frequently used than common nouns, it is

not surprising that in all languages where there is a difference between the inflection of common nouns and that of surnames, the latter is less differentiated:

<u>French</u>	salon	salons	Bourbon	Bourbons
	chevalier	chevaliers	Chevalier	Chevalier
<u>German</u>	cheval	chevaux	Cheval	Cheval
	Müller	Müller	Müller	Müllers
	Falke	Falken	Falke	Falkes
<u>Italian</u>	Korn	Körner	Korn	Korns
	cavallo	cavalli	Cavallo	Cavallo
<u>Polish</u>	król	królowie	Król	Królowie
	kupiec	kupcy	Kupiec	Kupcowie
<u>Spanish</u>	gołąb	gołąbie	Gołąb	Gołąbowie
	herrero	herreros	Herrero	Herreros or Herrero
	lápiz	lápices	López	López

In all the cases where there is a difference between the common nouns and the surnames, the inflection of the latter is simpler. The surnames are invariable, show fewer endings or no alternation.

As far as numerals are concerned, the higher a numeral is, the lower its frequency is, and this explains why in all Indo-European languages the suppletion always occurs in the number '1' (one-first), sometimes in the numeral '2' (two - second, but German zwei-zweiter) and never from the numeral '3' onwards (three - third, etc.).

Syntax: The personal pronouns, which are more often used than the substantives, can be combined with verbal forms in all three persons, while the nouns can be combined only with the forms of the third person. The verbal forms of the third person, which are used more frequently than those of other persons, can be combined both with the pronouns and the substantives, whereas the forms of the first and second persons can be used only with the pronouns. The indicative, which is more often used than the imperative, can be combined both with a nominative and an accusative, while the imperative can be used only with an accusative: you help him, but help him. The Latin preposition in can be combined with two cases, whereas the less frequently used preposition inter can be combined only with one case. The definite article is more frequently used than the indefinite one, and that is why the use of the former is more differentiated than that of the latter. Therefore, in any syntax of English, the chapter on the definite article is longer than that on the indefinite one.

Vocabulary: A verb like do has many more meanings than a rarely used verb like perceive. In other words, the meaning of a frequent word is more differentiated than that of a word which is seldom used. Loan words have fewer meanings in a foreign language than in the language of origin, e.g., chauffeur has only one meaning in English and more meanings in French. This is due to the fact that chauffeur is more frequently used in France than in the English speaking countries.

In addition, I would like to devote some words to suppletion, which consists in using more than one root within a paradigm. As

far as I know, it is a phenomenon which linguists only state but never try to account for. To my mind, suppletion is a particular case of the general law concerning the relationship between differentiation and frequency of linguistic elements. This allows us to understand the noteworthy fact that in different languages suppletion occurs in words of more or less the same meaning. It is mostly the question of the following categories of words:

the pronoun of the first person: I, me, we, us, Germ. ich, mich, wir, uns, Fr. je, me, nous, It. io, me, noi, ci, Russ. ja, menja, my, nas;

the pronoun of the second person: thou, you, Germ. du, ihr, Fr. tu, vous, It. tu, voi, vi, Russ. ty, vy;

the pronoun of the third person: he, they, Germ. er, sie, It. egli, essi, Russ. on, ego;

the word meaning 'every, all': Germ. jeder, alle, Fr. chaque, tous, It. ogni, tutti;

the noun meaning 'man, people': Germ. Mann, Leute, Fr. homme, gens, Russ. čelovek, ljudi;

the adjective meaning 'good, better': Germ. gut, besser, Fr. bon, meilleur, It. buono, migliore, Russ. chorošij, lučšij;

the adjective meaning 'bad, worse': Fr. mauvais, pire, It. malo, peggiore, Russ. plochoj, čuže;

the adjective meaning 'large, larger': It. grande, maggiore, Russ. velikij, bol'sij;

the adjective meaning 'small, smaller': Fr. petit, moindre, It. piccolo, minore, Russ. malj, meňšij;

the adverb meaning 'much, more': Germ. viel, mehr, Fr. beaucoup, plus, It. molto, più, Russ. mnogo, bol'se;

the adverb meaning 'little, less': Germ. wenig, minder, Fr. peu, moins, It. poco, meno, Russ. malo, menee;

the numeral meaning 'one, first': Germ. eins, erster, Fr. un, premier, It. uno, primo, Russ. odin, pervyj;

the numeral meaning 'two, second': Fr. deux, second, It. due, secondo, Russ. dva, vtoroj;

the verb meaning 'be': are, is, was, been, Germ. bin, ist, war, Fr. est, fut, été, It. è, fu, stato, Russ. est', byl;

the verb meaning 'go': go, went, Fr. va, aller, ira, It. va, andare, Russ. idet, šel.

That suppletion occurs in different Indo-European languages in a more or less parallel way is very easy to account for: the suppletion occurs only in the most frequent words and these are everywhere practically the same. If somebody doubted that there is a connection between suppletion and frequency, one fact more could be mentioned. The inflection of the French verb aller shows four roots: va, allait, ira, and fut (used sometimes in the meaning of 'alla'), but the only tense where two roots occur is the present indicative (vais, vas, va, allons, etc.), which is the most frequently used tense. The same applies to the German verb sein, cf. on the one hand the pres. ind. bin, bist, ist, etc. and on the other hand all other tenses, which are less often used and show no suppletion. The same applies to the English verb be, cf. on the one

hand the pres. ind. am, are, etc. and on the other hand the remaining tenses. It happens in different languages that pronouns show suppletion in the singular and no suppletion in other, less frequently used numbers, cf. she, her but they, them or Lat. ego, mihi but nos, nobis.

It is necessary to insist on the fact that suppletion is not a heritage from Proto-Indo-European, but comes into being in all periods of the history of Indo-European languages. Suffice it to say that suppletion occurs in Romance languages in some words that had a regular inflection in Latin. There was no suppletion in the Latin pronouns nos, vos, but in Italian one says noi - ci 'we - us', voi - vi 'ye - you', and It. vi - ivi. There was no suppletion in the Latin verbs vadere, ire, or ambulare, while the inflection of Fr. aller, Span. ir, or It. andare is suppletive. The Latin adjective magnus, major, maximus did not show suppletion, whereas Italian replaced magnus, major by grande, maggiore.

It is also very important to stress the fact that suppletion is not a privilege of Indo-European languages. E.g., the Hungarian inflection is on the whole more regular than that of most Indo-European languages, but nevertheless the most frequently used verb meaning 'be' is suppletive, cf. van 'is' but lesz 'will be'. The suffix of ordinal numerals is -dik, e.g., tíz 'ten' and tizedik 'tenth', but there is suppletion in the two lowest numerals: egy 'one' but első 'first', kettő 'two' but második 'second'. The same applies to sok 'much' and több 'more' or to the pronoun of the first person: én 'I', mi 'we', and bennünket 'us'.

Finally, it is necessary to mention that there are, of course, facts which contradict the law according to which the more frequently used linguistic elements are more differentiated than those seldom used, but it is very important that the majority of facts confirm the law in question and that this law, which applies to all languages of the world, allows us to bring to a common denominator a great many phenomena referring to spelling, phonetics, word formation, syntax, or vocabulary, which, at first glance, seem to be heterogeneous.

¹On Nov. 20, 1965, I gave, in the Paris Société de Linguistique, a lecture on La nature du supplétivisme, whose complete text appeared in Linguistics 28, 1966, pp. 82-89.

ON DETERMINING THE NUMBER OF STRATA IN LINGUISTIC STRUCTURE

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The stratificational school got its start largely from findings that the two-stratum schemes of post-Bloomfieldian models were inadequate to account economically for alternation phenomena. It was found that some languages at least have two separate layers of alternation between what were usually called morphemes and what were usually called phonemes. The situation (well discussed by Hockett [1]) could be handled by positing a morpho-phonemic level between the morphemic and the phonemic, with the so-called morphologically conditioned alternations intervening between morphemic and morphophonemic and the phonologically conditioned alternations between morphophonemic and phonemic. Some time later (about 1964) the term lexon was proposed for the units just referred to as morphemes, and the (morphologically conditioned) realizations of the lexons were now called morphemes, in keeping with some earlier uses of that term.

It was also found in the early period, when the label stratificational wasn't yet being used, that alternations also exist above the level of the lexons, for example those alternations treated in some models as transformations. Consistent treatment required that another stratum be posited above that of the lexons, and the result was the early form of stratificational grammar with the four strata called sememic, lexemic, morphemic, and phonemic.

During the past twenty years various other stratificational models have been proposed. In addition to the four-stratum scheme, which has perhaps been the most common, there have been models with as many as five [2] and even six [3] strata. The variation in the number of strata seen in different versions results from varying criteria for recognizing stratal distinctions. Besides the evidence from alternation, which can be variously interpreted, we have the evidence from (1) differences in tactic patterning, also having various interpretations, and (2) the type of structure which has been called the sign pattern.

Although it is not to be doubted that the three types of evidence tend to reinforce each other, we have to be careful not to follow that principle too simplistically. In particular, it is evidently not the case that for every layer of alternation we find different tactic patterns above and below. For example, if we attempt in

phonology to set up a tactics of morphophonemes separate from the tactics of phonemes, we find that the two have a great deal in common, and we suspect that perhaps we are not dealing with two separate tactic patterns after all--not two different patterns in the way that a phonological tactics is different from a morphological tactics. In keeping with such findings it has been proposed that a stratal system can have more than one layer of alternation, e.g. both above and below the tactics [4]. A related but different possibility is to allow alternations within tactic patterns; and just as we allow indefinitely many constructional layers within a tactic pattern, then we would accordingly have no limitation on the number of layers of alternation within a single stratal system.

But it would follow from such treatment that alternation can no longer be taken as evidence for stratal distinctions.

Further complications arise from the different possibilities available for interpreting tactic evidence. Differences in tactic patterning would seem to imply different tactic patterns, hence separate strata. But how much difference, and what kind of difference, is sufficient to justify the positing of two separate tactic patterns? It can be argued that tactic patterns must in any case be powerful enough to include coexistent differing constructions (requiring upward 'ands' within tactics) as well as various relationships which appear at least very similar to alternations. And if we allow upward 'ands' within a tactic pattern then we are allowing a tactic unit (construction, class, or element) to have two functions simultaneously. This makes possible the treatment of different, one might say conflicting, tactic configurations within a single tactic pattern.

This line of reasoning could lead to a model with fewer stratal distinctions than those recognized in recent versions of the theory. In particular, it appears that the morphemic and lexemic strata of recent versions may be combinable into a single stratal system.

In order to explore this view it is necessary to consider some of the data that have previously been used as evidence for recognizing separate morphemic and lexemic strata. One of my favorite pieces of evidence used to be the English verb phrase. According to the old argument I would now like to bring into question, it provides clear-cut evidence of a stratal distinction, as we have two quite different tactic configurations. To review the phenomenon, perfect tense is marked by 'have' and '-en' but these two parts of the realization are not continuous. Rather, the suffix '-en' appears on whatever verb follows, for example 'take', and 'have' gets a suffix, such as 3rd Sg. Pres. independently of the Perfect element, as in 'has taken'.

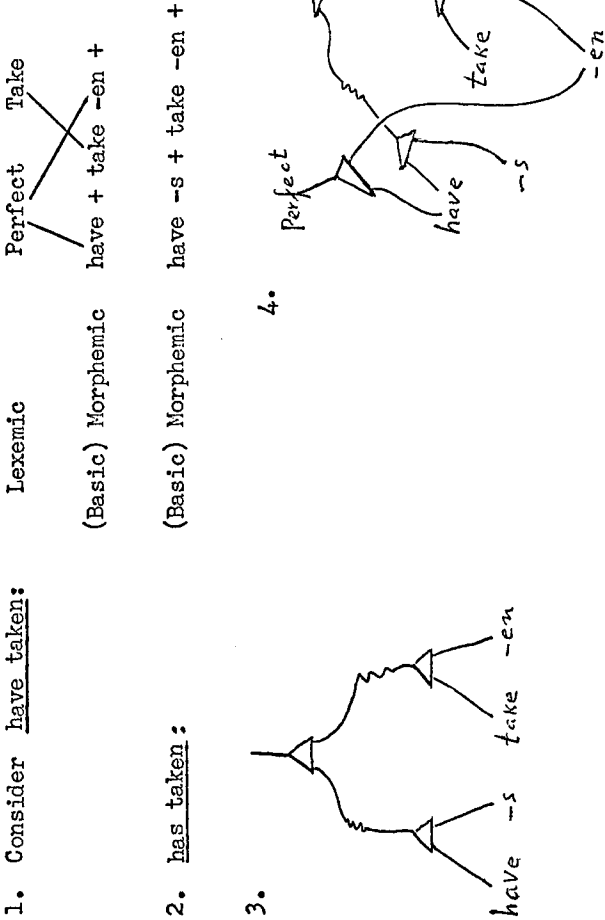


Figure 1

If we also have the progressive tense the following verb is 'be' and we get 'have been taking' with the '-en' suffix on the 'be' of the progressive and the second part of the progressive, the suffix '-ing', on the 'take'. That is, the progressive element also has the two parts of its realization occurring discontinuously. The passive marker works the same way; the first part is 'be' and the second is the suffix '-en', not on this 'be' but on whatever verb follows. We get, therefore, for perfect passive, 'have been taken'. The realizational discrepancy called anataxis in stratificational circles is involved here for these three elements. Another way to put it is that the three elements of a higher level have discontinuous realizations at the morphemic level.

Figures 1, 2, and 3 are machine-copied from the handout used in a paper presented to the Linguistic Society of America in 1969. The purpose of that paper was to show how the ordering phenomena can be treated in a relational network approach. Although it wasn't the point of that paper, I have used the data on various other occasions as evidence for a stratal distinction. Part 4 of Figure 1 shows that 'have' and '-en' have dual functions; each has a function in a morphotactic construction as well as a function as part of the realization of 'Perfect'. Figure 2a shows more of the relevant morphotactics, and Figure 2b has the 'Perfect' element added, together with diamond nodes.

Figure 3, also taken from the 1969 paper, shows with little numbers the relative sequence of activations in time through the network to produce the desired result. I will not comment on it further here.

As is well known to those who have followed the crooked path of progress of the stratificational school over the years, the diamond nodes came in as replacements for upward 'and' nodes at the points where tactic lines were intersecting with realizational lines. The diamond nodes act just like upward 'ands' for the encoding process but are slightly more complex for decoding. In micronotation we would have to add a little bit to the upward 'and' to make it function in the desired way for decoding, according to certain hypotheses of how decoding works. But for our present purposes it will be better to revert to the upward 'and'. Since we want to leave it an open question whether we are at a point of intersection of tactic lines and realizational lines. If we are going to entertain the possibility of including alternations within tactics then the lines involved in alternations are not necessarily perpendicular to those of the tactic pattern. We can also at this point leave the question of how the decoding process works unanswered. It was never fully answered anyway under the diamond hypothesis. Whatever we may have to add to certain upward 'ands'

later for decoding purposes may be added as justified by findings from studies of decoding. Meanwhile the use of upward 'ands' has the distinct notational advantage over diamonds that it does not

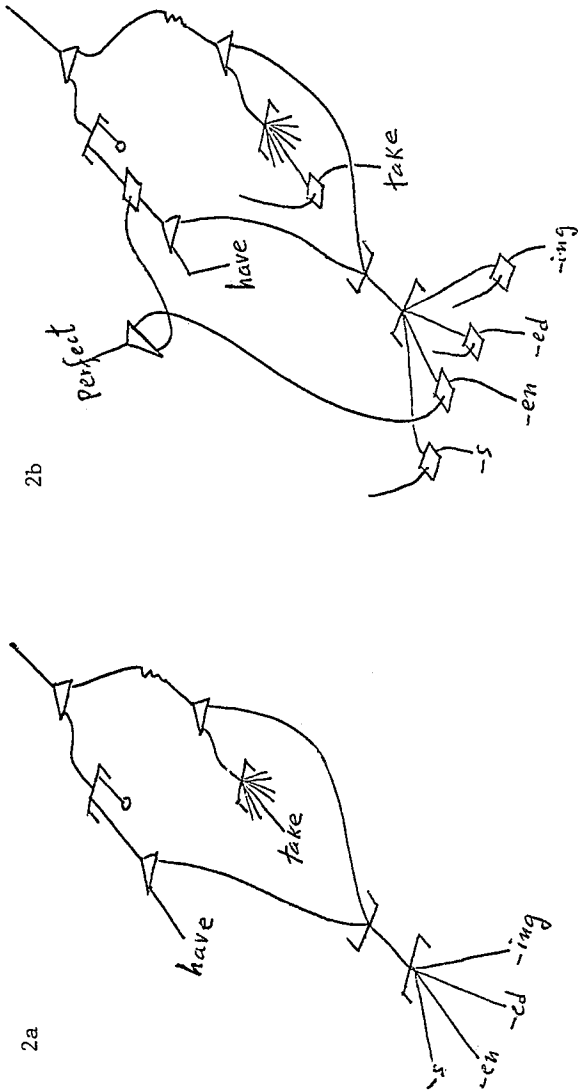


Figure 2

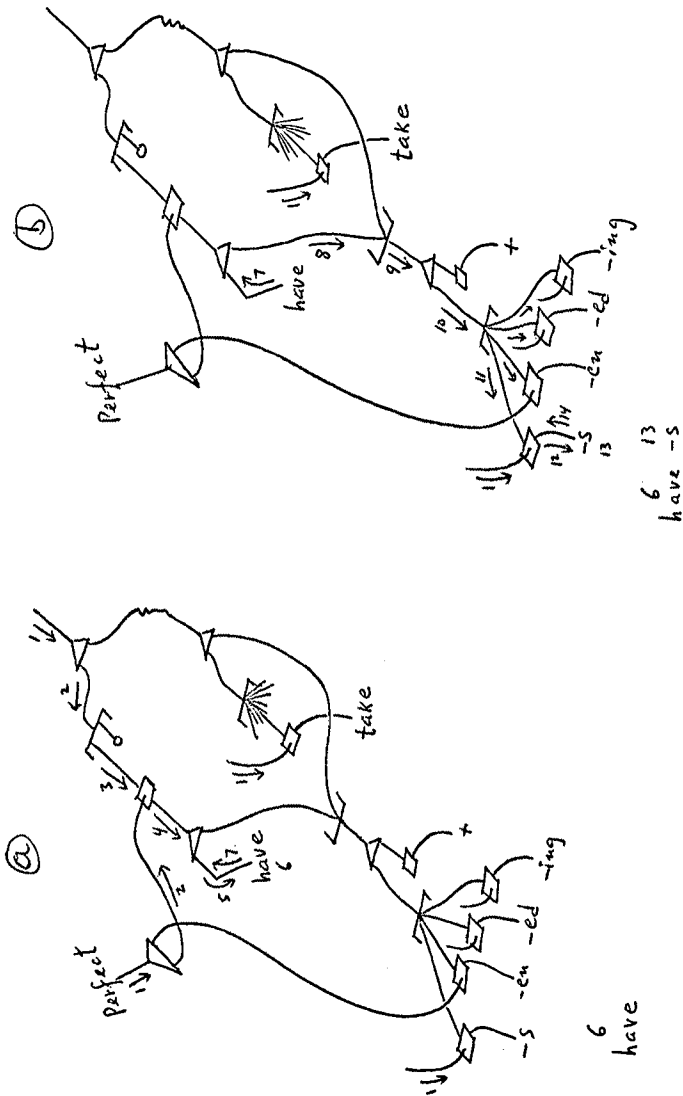


Figure 3, Part I

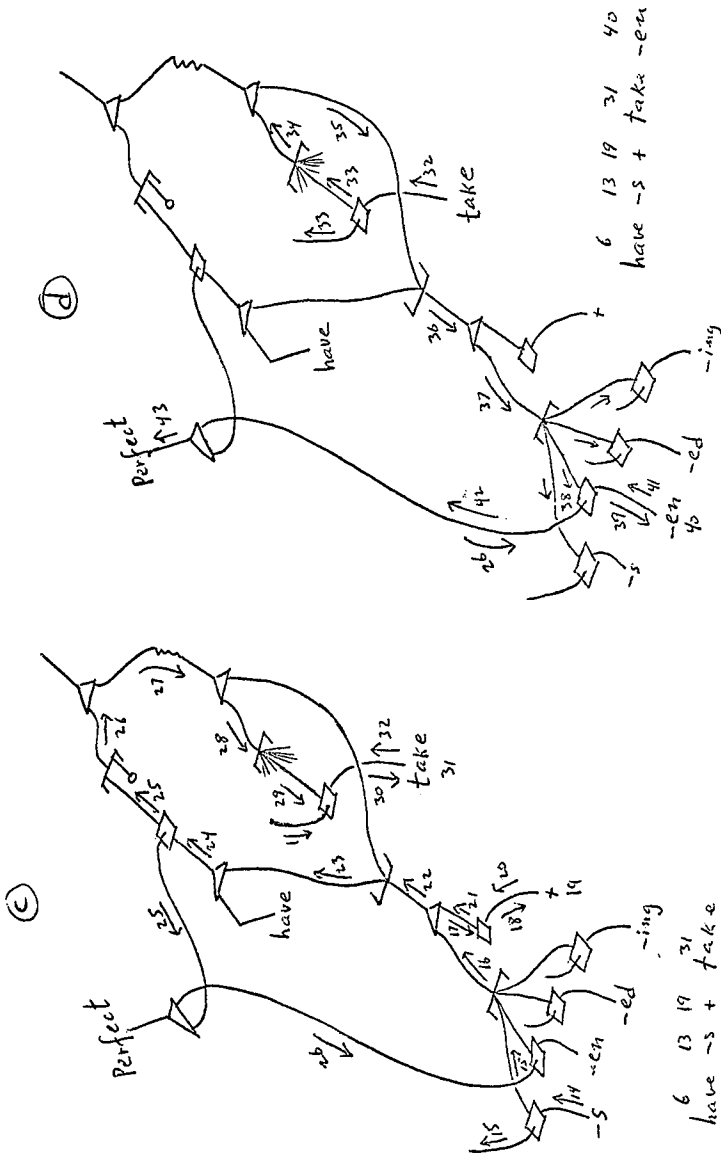


Figure 3, Part 2

influence us to perceive perpendicularity and stratification where none may be present.

We may accordingly revise Figure 2b just by replacing the diamonds with upward 'ands'. The result is Figure 4 (Figures 4, 5, and 6 are new for the present paper.) For the example in question, the difference between the diamonds and the upward 'ands' doesn't happen to affect the case. Either way, whether we are looking at Figure 2a or at Figure 4, we can interpret the situation as a fragment of tactics together with a part of a sign pattern. That is, the point labelled 'Perfect' together with its 'and' node may be taken as belonging in the sign pattern just above the morphotactics.

Note that this view is not different in 1982 from that of 1969. This point may perhaps be seen more clearly in Figure 5, adapted from the handout of 1969 as Figure 4 is adapted from Figure 2b, in which the progressive element is added. Here we see, and it was true already in the analysis of 1969, that the two conflicting tactic facts of (1) a verb phrase construction (that

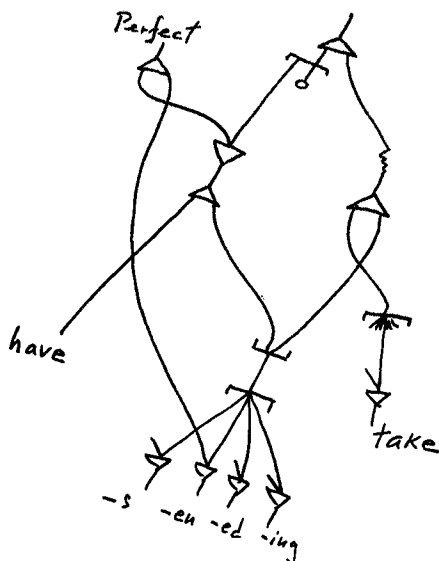


Figure 4

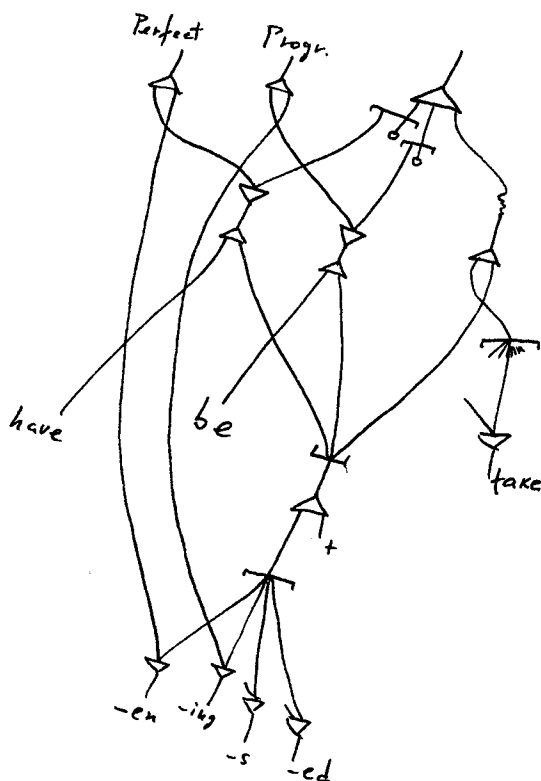


Figure 5

at the top of the figure) with optional perfect marker followed by optional progressive marker followed by main verb, and (2) the discontinuity of the representations of the perfect and progressive markers, are accounted for within a single tactic pattern. What makes it work is that the points of intersection of the tactics with the lines from the associated sign pattern have further tactic structure below them. This structural property was not allowed in the model of Outline of Stratificational Grammar [3], but it was introduced soon thereafter and is present, for example, in the version of Lockwood's textbook. But the implications of allowing these points of intersection to be in the middle of a tactic pattern, as it were, and not just at the bottom, have not been fully appreciated.

So crooked has our path been that only a year and a half later, at the Georgetown Roundtable of 1971, I once again used the example of the English verb phrase as evidence for a distinction

between the morphemic and lexemic strata [5]. Evidently I failed to appreciate what I had done in 1969.

A somewhat related example is shown as Figure 6. The figure accounts for the two facts which must somehow be accounted for, that (1) 'understand' functions as a unitary element in tactic constructions, and its meaning is not a composite of the meanings of 'under' and 'stand'; and (2) it nevertheless has the same element 'stand' as the verb 'stand', as shown by the past tense forms 'understood' and 'stood'. This is another example that has been used more than once as evidence for a stratal difference between the morphemic and lexemic strata. But we can do it all within a single stratum if we just allow the device used in Figure 6; which may be described in words as the compound morpheme or macromorpheme. This notion is not new either. It was discussed in a seminar at Yale in the late sixties, in which Peter Reich, among others, was a participant. For 'understand', we have a downward 'and' of the type seen in sign patterns, and one which has commonly been said to belong to the lexemic sign pattern, since the elements 'under' and 'stand' seem to be morphemes. But that view overlooks the fact that 'understand' does not represent a productive morphological construction. When it occurs, it occurs as a unit. It is not "constructed" by morphotactics or any other tactics. Therefore it would be a mistake to put the 'and' node of 'understand' above the tactics and to consider the 'under' and 'stand' of 'understand' to be two separate morphemes.

In choosing the data for this paper my aim has been to select some examples that have been used previously as evidence for the distinctness of the morphemic and lexemic strata. And so I turned

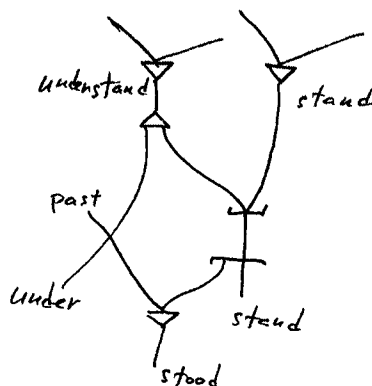


Figure 6

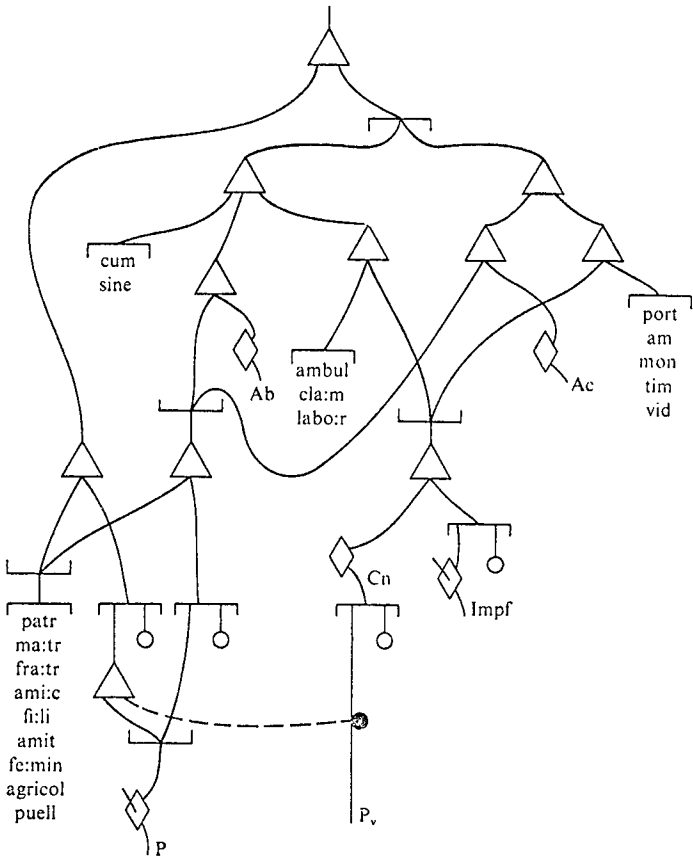


Figure 7 Latin Lexotactics

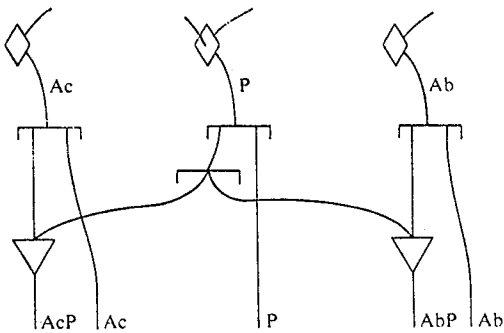


Figure 8 Portmanteau Realization of Case and Number

to David Lockwood's textbook to see what evidence he gives there, and I found to my surprise that there is not very much. In Chapter Two he gives the example of 'understand', which we have just considered. Later, in the chapter on Grammatical Phenomena, he uses some data from Latin. His catalysis is shown in the next four figures. In this case, it appears that the main reason for the division into morphemic and lexemic strata was the limitation of morphotactics to the morphological word as the largest tactic unit. But other versions of stratificational theory have not had such a limitation, and there appears to be no good reason for it.

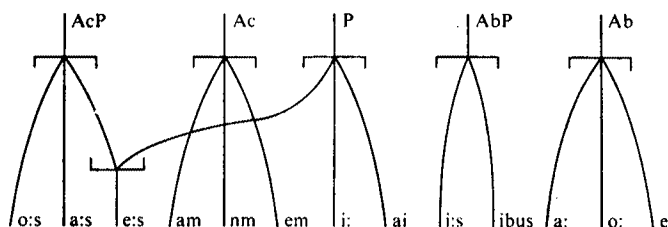


Figure 9 Lexonic Alternation Pattern for Case and Number

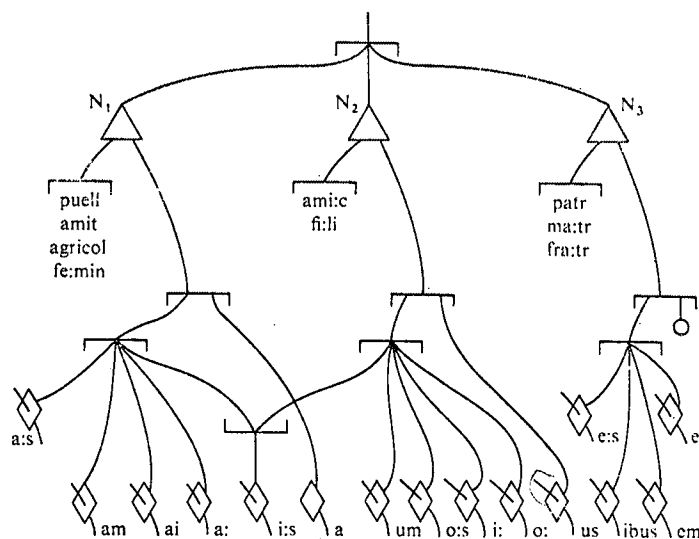


Figure 10 Latin Noun Morphotactics

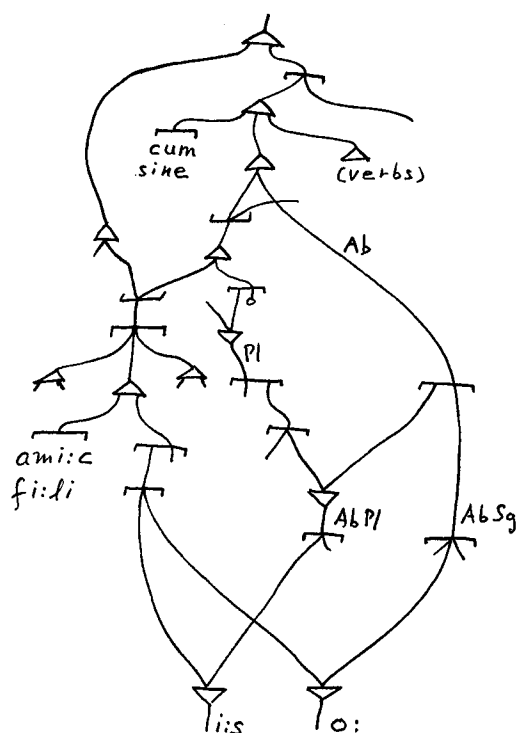


Figure 11

An alternative treatment, which follows Lockwood very closely, is shown as Figure 11. In this figure, which takes a representative cross-section through the figures of Lockwood's catalysis, all the data are accounted for within a single stratum.

Other cases not to be considered specifically here seem to involve conflicting tactic patterning or multiple tactic functions of a tactic unit. They can be handled by means of upward 'and' nodes within the tactic pattern. Not all practitioners of stratificational grammar have used upward 'ands' within tactics, although they are very close to enablers, which have not been objected to, as far as I know. It is conceivable that some might want a general policy that would disallow upward 'ands' within a tactics, except at points of intersection with realizational lines--that is, lines from outside the tactic pattern. I don't know whether that point of view is currently supported by anyone, but it seems rather arbitrary to me. If we do not allow upward 'ands' in a tactics, and if we also disallow enablers, which are quite closely related, then we are restricting tactic patterns to the equivalent of what have been called by the rather unfortunate but nevertheless widely used term "context-free

phrase structure grammars". There is no good reason that I know of to impose such a restriction on language. If we do impose it then we are faced with the consequences that whenever we find a tactic unit having a dual function, or wherever we find a tactic configuration, however isolated, which has dual tactic interpretations, we are forced to set up a separate tactic pattern, hence a separate stratum. The policy thus leads to a proliferation of strata.

This line of reasoning leads to the conclusion that differences of tactic patterning cannot by themselves be taken as evidence for stratal differences.

We have already made similar arguments with respect to alternations.

How then can we determine how many strata there are?

It may be that, just as most of the actual information content of a language is in the signs, it is the sign patterns which provide the chief evidence for stratification. Such a conclusion supports (and is supported by) the common intuitive impression to the effect that, for example, grammar and phonology are really quite different because they are dealing with two different kinds of units. What makes the two kinds of units different (even though they must be related to each other to belong in the same language)? It is the intervening sign pattern, in this case that which has generally been called the morphemic sign pattern. This sign pattern is what provides the relationship as well as the difference between these two kinds of "units". By contrast, the difference between phonemic and morphophonemic is not so great, as we are dealing here with what we intuitively consider the same kinds of units. We can back up the intuition by observing that no sign pattern intervenes between phonemes and morphophonemes. Similarly, with different tactic levels in grammar, as well as with different alternation levels such as those elsewhere treated by means of transformations, we are dealing with the "same kinds of units".

Following this line of reasoning we are led to the conclusion that the early evidence leading to the first proposals of stratificational grammar was actually the wrong kind of evidence for recognizing stratal distinctions. Not only do morphophonemes and phonemes both belong in the same stratal system, but also by the same line of reasoning, deep grammar and surface grammar belong in the same stratal system. What separates them is only alternations and minor differences of tactic patterning. The real stratal distinction separating the level of grammar from something higher is another sign pattern, separating the kinds of "units" dealt with in grammar from "units" of a quite different kind, namely concepts.

The possibility thus exists of an interpretation of linguistic stratification with just three stratal systems, the phonological, the lexico-grammatical, and the conceptual. If these three strata (and it's a big 'if') are comparable to the three main divisions recognized by others, then they are certainly justified in asking why it took me so long to see the light. I am happy to join them, and I only add that the first stratificationalist, Adolph Noreen, who originated the term 'sememe', declared in the very early years of this century that language has three strata, phonemic, morphemic, and sememic [6].

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 3. Sydney M. Lamb, Outline of Stratificational Grammar. Georgetown University Press, 1966.
 4. E.g., David Lockwood, Introduction to Stratificational Linguistics. New York: Harcourt Brace Jovanovitch, 1972. Pp. 120-123, 228-234.
 5. Sydney M. Lamb, The Crooked Path of Progress in Cognitive Linguistics. Georgetown Monograph Series on Language and Linguistics 24.99-123 (1971). Reprinted in Makkai and Lockwood, Readings (see Reference 2).
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III

PHONOLOGY

TOTALLY OBSTRUCTED SYLLABLES IN DYNAMIC PHONOLOGY

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In many languages of the world, we find a phenomenon in which a word is uttered with one or more syllables apparently lacking a vowel. In a study of slightly more than one hundred such languages, Bell (1978) found that in those instances in which it is not vocalic, the "syllabic" is most often a sonorant (a liquid or nasal), though cases of syllabic obstruents are also to be found.

Within traditional segmental and semisegmental phonological theory, however, there is a large problem with the very concept of a vowelless syllable--a problem that stems from the position of the prosodic feature. Among the segmental schools there has traditionally been a dichotomy between inherent and prosodic features. According to Jakobson, Fant, and Halle (1952: 13), for example, "The distinctive features are divided into two classes: 1. inherent and 2. prosodic. The latter are superposed upon the former and are lumped together with them into phonemes." (On this point, compare Trubetzkoy 1969: 182-97.)

Traditionally, the prosodic features, such as stress and pitch, have been "lumped together" with the nuclear vowel of the syllable. Thus, for example, the difference between the English noun permit and the verb permit can be described in terms of the segmental placement of the prosodic feature of stress. In the noun, the feature is included with the nuclear vowel of the first syllable; and in the verb, it is included with the nuclear vowel of the second.

To be sure, there is a rather large gap between theory and practice in this matter. Although Chomsky and Halle (1968: 68) state that "This unique treatment of the feature 'stress' reflects the fact that stress is a prosodic feature, i.e., a feature whose domain extends over sequences that are longer than a word;" nonetheless, they go on to formulate their stress placement rules in terms of the prosodic feature's restriction to the nuclear vowel.

Such an approach creates a problem when it comes to vowelless syllables. The fact is, that even in English, a vowelless syllable can maintain contrastive pitch. For example, let us consider the sentence This is London, where the word London is pronounced with a syllabic nasal /lʌndn/. By placing falling pitch on the syllabic nasal, we utter a statement; and by placing rising pitch on this syllabic nasal, we utter a question. Thus, somewhere in this syllable we must place a syllabic

prosodic feature of pitch, but there is no vowel into which this feature can be "lumped."

Perhaps the most straightforward manner of alleviating this problem in the generative approach lies in the underlying segment. We can posit an underlying vowel /ə/ with various inherent features, including the feature /-pitch/. In cases of questions, we simply write a rule changing the specification to /+pitch/.

This approach to the problem is taken by Wurzel (1970: 185-93) in his generative treatment of the German vowelless syllable. Because the phenomenon in German arose historically from the centralization of the unstressed vowel, the underlying vowel coincidentally maintains the inherent features of /ə/.

When the corpus is expanded to include the instances of vowelless syllables in Mandarin Chinese, however, this tactic falls apart. In Chinese, syllables maintain prosodic tone features, which, according to this approach, would be assigned to the underlying vowel in a vowelless syllable. As an example, let us consider the word /sʒ/ 'four' with falling tone over a syllable that shifts directly from voiceless sibilance to voiced sibilance without the possibility of a break that could be interpreted as a vowel.

The question in generative phonology would, then, concern the position of the underlying vowel. The most obvious choice of position would be between the two obstruents. This choice is not open to us, though, because to place it here would make the word end in a single /z/, which, according to Fenn and Tewksbury (1967: ix--compare also Hockett 1947 on the Peiping dialect), cannot be done in this language, in spite of the fact that the /z/ in combination may appear in this final position, as we find in /tsʒ/ 'occasion', for example (Fenn and Tewksbury 1967: xvi). This leaves us only with the choice of the sequence /szV/. Unfortunately for the generative segmental approach, this sequence is not acceptable in this language either (again, see Fenn and Tewksbury 1967: xvi-xvii). Thus, there is no way within the morpheme structure constraints of Chinese to posit a vowel for /sʒ/ in the underlying string.

Even where an acceptable consonant cluster can be found, the underlying-vowel hypothesis breaks down. For example, the word /dʒ/ 'word' has falling tone over a syllable that begins with voiced (unaspirated) closure and releases directly into homorganic voiced sibilance. It would indeed be possible to posit an underlying structure in which /dʒ/ forms the initial (but not the final!) consonant cluster with some vowel following, as we find in the word /dʒaʊ/ 'be early', for instance.

The problem with the analysis relative to /dʒ/ is that there is not a single vowel that we can posit on the underlying level with specific instructions to delete in the transformation to the surface. Of course, we can make up a vowel that appears nowhere else in the language; but where this tactic of abstractness has been employed previously (as in Hyman 1970), there has been some justification for the vowel's feature

specifications, some rational reason for positing a particular underlying vowel. Here, on the other hand, there is no such justification and consequently no connection between such an analysis and linguistic reality.

The reason for generative phonology's inability to account for the vowelless syllable is quite transparent: The model is far too reliant upon the speech segment. Somewhere on the underlying level, a feature must be attached to a segment; and where no segment exists, one must be created. But of course, the prosodic feature is not in theory limited to a segment. The dichotomy here between phonological theory and the totally segmental generative model has, then, created an enigmatic obstacle to analysis and description.

Not all approaches are so absolutely segmental, though. The London school approach to prosodic analysis founded by Firth (1948), for example, maintains a semisegmental model providing the analyst with a method of describing features that goes beyond the confines of the segmental structure. In this approach we find a structural portion with consonantal and vocalic segments, or phonematic units, devoid of features except for those that establish consonantality and vocality (compare Mitchell 1975). Layed over the structural portion is the systemic portion made up of prosodic features "determined mutually or shared by consonants and vowels" (Mitchell 1975: 35).

In the systemic approach, one characteristic must be taken into consideration: "The vowel is the essential element of the structure of a syllable; every syllable must have a vowel" (Berry 1977: 81). Thus, perhaps the most straightforward way of describing "vowelless syllables" in prosodic analysis is through the principle of polysystematicity--that "in phonology we should recognize systems of unitary distinctions appropriate to initial consonantal and vocalic elements in contrast with those applicable to final and variously medial counterparts, or relevant to tonic syllables in opposition to atonic syllables, or again apt to short vowels versus long vowels, etc, and this to the utmost degree of refinement consistent with the facts in given cases" (Mitchell 1975: 34).

In a polysystemic analysis, we would simply reinterpret the /z/ in the Chinese example as a vocalic phonematic segment. While this interpretation may in principle be in keeping with the understandable imposition of the analyst upon the analysis (as pointed out by Robins--1957), in actual practice it is aberrantly and even abhorrently abstract in the systemic approach. After all, if an obstruent segment as obviously consonantal as /z/ is to be interpreted as a vowel, then why should we not rather interpret the /s/ as the vowel? This could be generalized by saying that any phonematic segment preceding a lone /z/ is a vowel--that is, /s/ and /d/. Indeed, this modification would conveniently render the word /tsʒ/ 'occasion' (cited above) as a CVC syllable (an analysis that would lead to a reinterpretation of the morpheme structure constraints on final single /z/).

The drawback in this analysis should be clear. Declaring a consonant to be a vowel by polysystematicity for the convenience of the analysis in essence leads to an abstract interpretation, if not to an underlying level of segmentation. In either case, it is not prosodic analysis at all, and it certainly does not represent "the utmost degree of refinement consistent with the facts."

An alternative prosodic analysis would include a vocalic phonematic segment at random into the structural syntagm. For example, we could posit a CCV structure and leave the prosodic feature of sibilance to cover the vowel as falling tone covers the consonants. This analysis faces the very same dilemma as that facing the generative attempt above. In essence, we again become abstract and posit an underlying vowel somewhere where, again, it cannot be justified elsewhere in the language. Such an analysis is neither systemic nor indeed systematic.

What is needed in the prosodic analysis is some way of bypassing the structural portion with its bothersome phonematic segments. If we could do this, then we could simply allow falling tone to coincide with sibilance. Such an approach, however, is blocked by the segmental component of this semisegmental model. If we were indeed to eliminate the segmental component and were to have all features realized prosodically, then we would in fact have a dynamic analysis as described below, a method that certainly has its Firthian influences (see Griffen in press a).

Prosodic analysis, though, is not the only semisegmental model. There is also stratificational grammar, which (as we see in Lockwood 1973, 1975) can make use of a quasi-stepmatrix to allow features properly to overlap. Getting to the quasi-stepmatrix, however, may in itself be an insurmountable problem, as pointed out in Griffen 1975.

On the phonemic stratum (as in Lockwood 1972, for example) or on whatever equivalent stratum, substratum, or level may take its place in any particular model, the falling tone of the example would have to couple with the phonotactics somewhere in the syllable. If it were to join directly to the vocalic line of the tactic pattern, we would face the same problems as those above.

Stratificational grammar, however, is more flexible than is prosodic analysis. We can in fact attach the node over the entire syllable, with provision made for a syllable type with no vowel. We accomplish this by connecting the diamond node with the realizational prosodic input to the tactic line above an ordered-and node conjoining two (or more) consonants.

This solves the problem from the point of view of a neat, nonabstract analysis. Unfortunately, it overlooks a vital aspect of tone: Tone and other syllable prosodies are not consonantal, to be placed in dominance over obstructions on the syllable; rather, they are vocalic phenomena produced with nonconsonantal vocalic articulators and producing nonconsonantal vocalic effects acoustically. To say that there is no vowel

in the syllable is to overlook the vocalic nature of the syllable prosody. In effect, the stratificational analysis makes the same mistake as does the prosodic analysis in arbitrarily classifying vocalicity as consonantality for the sake of the analysis.

So where is this vowel that must be there but at the same time cannot be there? When faced with such a dilemma, the phonologist following the inner approach (Jakobson and Halle 1971) should examine the phonetic evidence.

When we do examine the evidence from experimental phonetics, we need not look very deeply to find that speech is not made up of discrete consonantal and vocalic segments, one following another in neat progression. Rather, the features of speech are arranged in accordance with the principle of dynamic coarticulatory constraint--certain features and feature types constrain or obstruct others with which they are simultaneously articulated over time. Indeed, even Anderson (1974: 5-7) recognizes this natural phonetic fact and acknowledges the reality of the dynamic principle, though he chooses deliberately to ignore it for the sake of tradition, notional segmentalism, and generative transformationalism.

When speech is analyzed from the more realistic dynamic perspective (as called for by Curtis 1954), certain relationships between "consonants" and "vowels" become apparent. In his working articulatory model, for example, Mermelstein notes the following working principles:

- (1) The midsagittal vocal-tract outline is modeled in terms of nine selected variables describing the position of the participating articulators.

- (2) Stationary vowels are represented in terms of four variables, two describing tongue-body position, one the jaw position, and one the lip position. Movement from vowel to vowel, expressed as changes in the variable values is slow and precisely controlled.

- (3) Representation of consonants requires additional control of tongue-tip elevation, lip height, and velar opening. Tongue body or jaw closure is specified by the variables pertinent to vowels.

- (4) Consonants are not defined directly in terms of variable values but by constraints on articulator position relative to the fixed structures. Articulators independent of the specific constraints are free to take on positions independent of the consonant under production subject to the requirement that they do not otherwise constrict the vocal tract.

- (5) Stop consonants are released by rapid movement of the constricting articulator. (1973: 1081-2)

Taking such principles of dynamic phonetics together, I have constructed a model of dynamic phonology in which syllabic (vocalic) features constrain laryngeal features, and these are

constrained by obstruational (consonantal) features in an intricate hierarchy of dynamic constraint. For our purposes here, the salient point is that the syllabic vowel is produced simultaneously with and constrained by the consonantal obstructions within the syllabic frame, as illustrated in figure 1. As the jagged lines are intended to imply, the beginning and ending points of features are not universal, though the overall hierarchical relationships (with some limitation and variation) are applicable in all cases.

Once we break down the syllable into its natural organization, the analysis of this supposedly "vowelless syllable" becomes obvious. Certainly, there is a vowel, for the tongue-body, lips, and jaw must indeed be in some position and must consequently produce some formant frequency pattern. This can be verified impressionistically by pronouncing the Chinese word /sɿ/ first with the tongue body in a high front position and then with it in a low back position. The sounds are different, so there is a vowel present, although it is constrained by obstruction features over its total dynamic length.

In what is properly termed the totally obstructed syllable, however, precisely what vowel it is that is produced and perceived is of no phonological pertinence. Therefore, in marking the phonological syllabic frame, we simply leave out features relating to tongue-body position, lip protrusion, and jaw height. We should note, though, that this practice makes use of phonological marking within a phonetic structure and demonstrates that this dynamic phonology is indeed functional dynamic phonetics (see Griffen in press b).

On the other hand, the falling tone of /sɿ/ is phonologically pertinent and is therefore marked in the syllabic frame as a syllable prosody, an implementation of finer vocalic or laryngeal features over the duration of the syllable. The obstructions, then, are simply realized in their natural function of constraint over the range of the entire syllable, as illustrated in figure 2.

Such an analysis is simple, straightforward, and, above all, completely consistent with phonetic reality. There is no need for abstract levels or for unrealistic designations, because we have conducted our analysis as true to phonetic reality as possible, holding to a minimum the preconceived notions of the analyst and extending to the maximum the functional use of the phonetic structure.

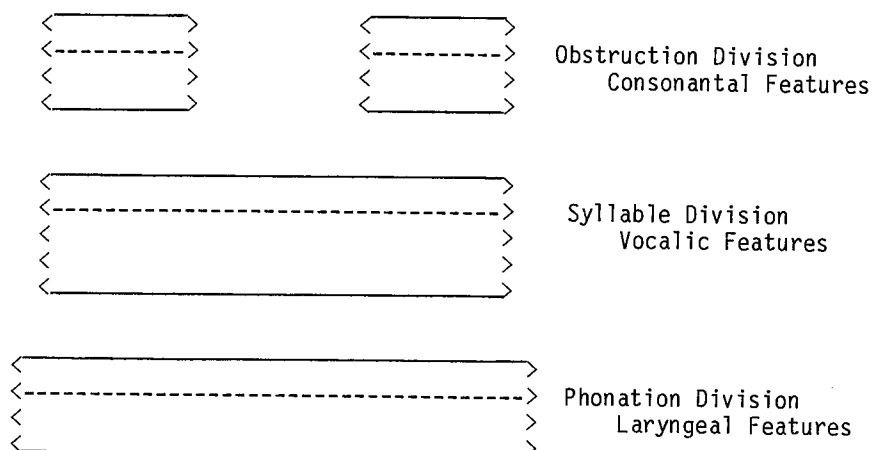


Figure 1: Syllabic Frame

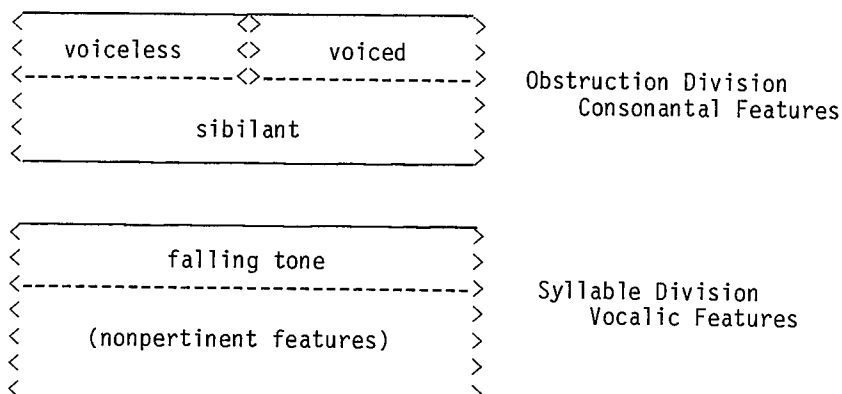


Figure 2: Dynamic Analysis of Mandarin Chinese /sʐ/

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PHONOLOGICAL FEATURES OF SELECTED COLUMBIAN LANGUAGES

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Last year, while conducting a phonology workshop for seven teams of the Summer Institute of Linguistics at the Lomalinda base in Colombia, South America, I found one insight to be of recurring assistance in the solving of the many varied problems we were facing. The one theme I heard myself humming over and over was that it is important to discover at what level (of the phonological hierarchy) an item or component is functioning. The field workers were, in general, well aware of allophonic conditioning at the level of phonemes (i.e., segmental variation) and they knew that such things as pitch, stress and length could condition segmentals, but they were not aware of the necessity of determining at which level (i.e. over what span) those suprasegmentals occurred. In addition, the possibility that one feature--such as pitch--could function at more than one level was virtually ignored.

In 1954, Kenneth L. Pike suggested (Solution C, Chapter 9 of Language in Relation to a Unified Theory of the Structure of Human Behavior, 1st edition) that suprasegmentals could be described as components of various phonological units which were arranged in a hierarchy. Such units might be, for instance, phoneme, syllable, phonological word, phonological phrase. Since that time, a number of descriptions--many authored by Eunice V. Pike--have appeared, utilizing this approach. A brief summary of this descriptive method can be seen in a Chapter entitled "Phonology," by E. V. Pike (in the volume Tagmemics: Aspects of the Field, edited by Ruth M. Brend and Kenneth L. Pike, Mouton, 1976). Also included in that chapter is an extensive bibliography of works utilizing this approach. The only article by Pike himself utilizing Solution C is, so far as I am aware, one entitled "Phonological Hierarchy in a Four-Cell Tagmemic Representation from Discourse to Phoneme Class," which is to appear in a volume of papers read at the Conference on Linguistics & the Humanities, U. of Texas at Arlington, March, 1981. (The volume is currently in press with the Summer Institute of Linguistics, and is edited by Robert E. Longacre and myself.) As I have stated, suprasegmentals are considered to be components of units in the phonological hierarchy. This approach allows for the treatment of pitch, for example, to be a component of more than one unit--e.g., of the phonological word (as tone) and of the phonological phrase (as intonation).

The Colombian languages studied in the workshop gave impressive evidence as to the usefulness of this analytical method. In Guahibo, Riena Kondo found a complicated system of vowel lengthening and shortening which resisted analysis for many years. Only after discovering the conditioning of length within syllables and phonological words was she able to find a solution which covered all of the data. Guahibo has both long and short vowels, contrastively. In addition, however, vowel length occurs when two identical short vowels come together at a juxtaposition of phonological word nucleus and affix. For example:

tómara 'town' + anë 'plural' > tomarānë

However, if the affix is composed of more than one syllable, lengthening does not occur at the affix-stem border, but rather, the two short vowels are rearticulated: e.g.

apo 'neg' + ojoba 'he sharpened' > apoojoba

This is only one instance of where lengthening occurs, but other instances are too complicated to explain in the short time available.

In Piapoco, Delores Klumpp found she needed to postulate a phonological unit between the syllable and the phonological word--termed a 'foot.' Within a foot, all the nuclear syllables must bear either heavy stress or light stress--a mixture of heavy and light stressed syllables is not permitted. Words, however, which consist of more than one foot may contain feet of the two types--e.g., nutānā catēnā 'that I may write'.

Epeña Pedé has contrastive nasal and oral vowels. In addition, however, there is the interesting phenomenon of 'nasal spreading'--resulting in derived nasal vowels, and nasal allophones of a number of consonants. Philip Harms found that nasality is best described as a component of the syllable. Basically nasal syllables contain a nasal vowel nucleus and a pre-marginal consonant which is manifested by a nasal allophone if it has one. E.g., p^haibā [p^haimā] 'black'; ipadā [ɾipānā] 'bird', epeñā [ʔepēñā] 'people'; sē [sē] 'tired'.

In addition, however, nasal spreading results in derived nasal syllables. Once a basic nasal syllable occurs, nasality spreads toward the end of the word until it is stopped either by a word (and, in some cases, by an affix) border, or by a syllable whose first consonant is a stop, s, č or ř.

Examples of nasal spreading stopping at a word boundary are as follows: peñōra [peñōñā] 'guagua' (a groundhog-like animal); dāwe [nawē] 'mother'.

When nasality meets a stop, s, č or ř within a phonological word, however, the interrupting sound is prenasalized and nasality ceases to spread as in: hōp^hhe [hōmp^hhe] 'a fish', āig⁺ [ʔāĩŋg⁺] 'daughter-in-law'; ɾ b⁺ s⁺ [ɾĩmb⁺s⁺]

'neck', wāithe [wāĩⁿthe] 'go (future)'.

The field worker found that once he was able to determine basic versus derived nasal syllables, and apply the rules of spreading within syllables and within words, he was able to account for all nasality.

In Paez, Florence Gerdel at first had to postulate inherent pitch-stress on only a few suffixes which invariably--but seemingly unpredictably--carried pitch-stress (which otherwise occurred only on stems). However, once phonological levels of word, phrase and paragraph were postulated (and their inter-relationships with grammatical and semantic systems were observed) the pitch-stress system became clear, with much of it completely predictable. It became no longer necessary to postulate inherent stress on certain affixes. Rather, it was found that these particular suffixes always occurred at the nucleus of the phonological phrase, and received sentence pitch-stress. (In general, the nucleus of the phonological phrase falls on a verb, whose stem receives extra stress and extra high pitch.) When these affixes occur they fall in the phrase nucleus. All of them could be loosely labeled 'emphatic.' Some of these affixes are: yux 'certainly', baa-na 'probably', š'aa 'just', ii 'intens'.

When attempting to analyze Coreguaje, Fran Gralow had vacillated for a long time between postulating a system which was primarily stress, and one which was primarily tonal. When considering it as tonal, she had a number of unexplained high-tone syllables which sometimes varied to a high-low glide, and sometimes were low. She knew that the glide was related to the syllable's occurrence in a declarative sentence. However, once it was established that stress was predictable in relation to its occurrence in the grammatical stem, and when pitch was examined as to its function on two different levels--the phonological syllable, and the phonological phrase--the entire system became apparent. At the syllable level, only two tones were necessary--high and low. In addition, pitch functions at the phonological phrase level (as a kind of intonation). I will illustrate only one part of the system here. Coreguaje sentences are most commonly composed of two phrases, the first a dependent, non-final type and the second a final one. In addition, there is at times a post-final 'tag' which usually comprises a repetition of some item in the final phrase--or a comment on such an item. The nucleus of a final phrase in a declarative sentence carries a pronounced high-low intonational glide, and the pitch continues to fall over the balance of the phrase. Syllables which otherwise bear high tone carry high-low tone when in the nucleus. The final 'tag' is rapid, and very low pitched. Both high and low toned syllables are very low when occurring in the tag. Examples: ñóom 'he shows' + jai 'witch doctor' > ñóom+jai; jaina 'witch doctors' + masime 'they know' + jahé 'banisterium' + uk'huna 'those who drink' > jaina masime, jahe uk'huna 'witch doctors know, those who drink banisterium'.

Many more problems were successfully solved once a phonological hierarchy was postulated, but perhaps the above are sufficient to demonstrate the usefulness of this approach.

Ample discussion and details of the phenomena discussed here can be found in a volume entitled *Selected Colombian Languages*, edited by myself— in press with the Summer Institute of Linguistics.

THE PRINCIPLED PHONOLOGY OF MALTESE VERBS

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1. Introduction

The main purpose of this paper is to present an alternative to the analysis of the phonology of Maltese Arabic verbs presented by Michael Brame (1972). A second purpose is to argue that my analysis is better because it is *principled*. The principle is *no (extrinsic) rule order* (Vennemann 1971). After a brief introduction, we will look at Brame's analysis (§2), and then at my alternative (§3). Then some comparisons will be made, and discussed (§4).

Probably fewer and fewer linguists believe in rule ordering, and the arguments against it are now almost innumerable (see, for example, Koutsoudas 1976, Pullum 1979, and recently Coates 1982). The present paper was inspired by the review by D. Odden (1979) of Hooper 1976, in which Brame's analysis of Maltese is held up as showing that "no adequate theory of phonology can completely exclude the cycle" (Odden, 441), a particularly complex type of rule ordering. Brame's analysis, in order to employ the cycle in the prediction of stress in the basic forms of Maltese verbs, turns out to employ ten arbitrarily ordered rules, and one of these an absolute neutralization. Its basis, a rule of antepenultimate stress, is virtually unevicenced in Maltese pronunciations!

Still, Odden and others who doubt the possibility of an alternative, unordered rule understanding of the Maltese facts perhaps deserve a reply, if only for the record. So my main purpose is just to present this alternative.

My second purpose is to show that the unordered rule analysis of Maltese is better science than Brame's. The requirement by science of falsifiable hypotheses is well known to most of us, if not to Odden or to others, e.g. Goldsmith (1979:221), who thinks that "one important way to advance phonological theory is to open the formal floodgates as wide as possible." Odden (440) considers the no rule order principle to be a part of "the goal of impoverishing phonological theory". Such a comment shows either lack of concern for scientific method, or, at least, ignorance of the fact that there is no lack of possible analyses of any reasonably complete body of language data. There may easily be at least as many analyses as analysts. The trick is to find the viable one(s), as fast as possible. This is achieved by preferring the more falsifiable (principled, limited, constrained) ones. But this is not the place to review scientific method (a good recent source is Rescher 1978). The method is, I hope, reasonably well illustrated in the conclusion intended from what I wish to show here: given

two analyses of the phonology of Maltese verbs, one limited by the principle of no rule order, and the other limited only by the ingenuity of its author, the former will be preferred, all other things being equal. I must of course leave to the reader a judgement of whether "all other things are equal", by which we mean, to be brief, attaining observational adequacy, with all the right generalizations clearly and precisely expressed, and with the minimum of theoretical mechanism. Indeed, I consider the present analysis to make fewer unempirical claims (another important principle would be to reduce these to zero), and it certainly has fewer rules, shallower derivations, and less theoretical mechanism. It also makes some empirical claims absent in transformational works.

2. Brame 1972

We consider here only the data presented by Brame 1972, as he presented them.¹ His analysis involves the following ten rules, which I will briefly illustrate in a few of his derivations. Brame assumes rules providing the following stem-shapes for the triradical roots: CVCVC in the perfect and CCVC in the imperfect. The particular vowels of stems are lexically determined; six basic types are traditionally recognized for Maltese (Saydon 1958).

(1) Brame's rules for Maltese

- | | |
|--|-----------------------------------|
| a. [+seg] C _j VC _j → [+seg] VC _j C _j | Identical Consonant
Metathesis |
| b. V → ∅ / V __ CC | Truncation |
| c. V → [+str] / __ C ₀ ((VC)VC ₀ ¹)# | Stress Assignment |
| d. i → o / $\left\{ \begin{array}{l} \text{oC}_0 \text{ —} \\ \text{—} \left[\begin{array}{l} +\text{cons} \\ +\text{son} \\ -\text{cor} \end{array} \right] \text{C}_0\text{o} \end{array} \right\}$ | Vowel Harmony |
| e. CRVVCV → CVRCV
(R is one of the sonorant
consonants) | Metathesis |
| f. $\left[\begin{array}{c} \text{V} \\ -\text{str} \end{array} \right] \rightarrow \emptyset / \text{ __ CV}$ | Apocope |
| g. ∅ → i / # __ RC | Prothesis |
| h. i → a / __ $\left[\begin{array}{l} +\text{cons} \\ +\text{back} \\ +\text{low} \end{array} \right]$ | Guttural Assimilation |
| i. i → e / $\acute{\text{V}}\text{C}_0 \text{ __}$ | Vowel Reduction |
| j. $\int \rightarrow \text{a}$ | Absolute Neutralization |

(2) Derivations with these rules

- | | | | |
|----|--------------|--|----------------|
| a. | hataf-it | | = Brame's (11) |
| | hátaf-it | Stress Assignment | |
| | hát'f-it | Apocope | |
| | hát'f-et | Vowel Reduction | |
| b. | korob-it | | = (14) |
| | kórob-it | Stress Assignment | |
| | kórob-ot | Vowel Harmony | |
| | kórb-ot | Apocope | |
| c. | ni-zboh-ik | | = (19) |
| | ní-zboh-ik | Stress Assignment | |
| | ní-zboh-ok | Vowel Harmony | |
| | ní-zb'ok | Apocope | |
| d. | ni-hrab | | = (21) |
| | ní-hrab | Stress Assignment | |
| | na-hrab | Guttural Assimilation | |
| e. | laʔat-t | | = (24) |
| | laʔát-t | Stress Assignment | |
| | lʔát-t | Apocope | |
| | ilʔát-t | Prothesis | |
| f. | [[ni-tlqb]u] | | = (31) |
| | ní-tlob | Stress Assignment | |
| | ní-tolb u | Metathesis | |
| | ní-tólb-u | Stress Assignment | |
| g. | ʔarar | | = (36) |
| | ʔaarr | I.C. Metathesis | |
| | ʔarr | Truncation | |
| | ʔárr | Stress Assignment | |
| h. | ti-hbob | | = (24) |
| | ti-hóbb | I.C. Metathesis | |
| | ti-hóbb | Stress Assignment | |
| | t-hóbb | Apocope | |
| i. | [[ni-ʔʊod]u] | | = (48) |
| | ní-ʔʊod | Stress Assignment | |
| | nó-ʔʊod-u | Vowel Harmony | |
| | nó-ʔoʊd-u | Metathesis | |
| | nó-ʔóʊd-u | Stress Assignment | |
| | nó-ʔóod-u | Absolute Neutralization
and "later rules" | |

The derivations (2)a-i illustrate the ten rules proposed in Brame 1972. It is not possible to discuss Brame's analysis in much detail here. The following brief comments review the derivations to give an idea of the ingenuity and intricacy of the analysis.

In (2)a we see how Stress Assignment precedes Apocope, an extrinsic ordering. It also feeds Vowel Reduction. In (2)b it seems that Vowel Harmony does not have to be ordered before

Apocope, since suffixal o could be the result of harmony with the stem vowel left after Apocope. But Vowel Harmony bleeds Vowel Reduction (changing to o an i which would be 'reduced' to e), an extrinsic ordering. In (2)c, however, the only stem vowel of an imperfect stem drops out, so we see that harmony has to precede Apocope, another extrinsic ordering. Note that regressive Vowel Harmony does not apply across the consonant z of the stem.

(2)d simply illustrates Guttural Assimilation, and (2)e illustrates Prothesis. Vowel Harmony must precede the former, as shown by e.g. tô-hlom (Brame, 33). A case like (2)f motivates the Metathesis rule, and the cyclic ordering of Stress Assignment. Without the cycle and the internal bracketing in this word, the stress rule would give nítlobu, which Metathesis would make nítolbu.² Brame says of the secondary stress he derives here that "one tends to hear it," but "if no weakened stress is actually present in the surface representation, however, we need only propose a further rule that eliminates the nonphonetic material." (p. 41n) Also in (2)f we see that Metathesis must be ordered before the second --but not the first!-- application of the stress rule.

(2)g/h show the need for the rule of Identical Consonant Metathesis, which must apply before the stress rule, another extrinsic ordering. The rule needs the feature '+seg', otherwise would affect e.g. ti-tla? giving ittla? (Brame, 43n). In the perfect, I.C. Metathesis causes a necessity for the rule of Truncation --apparently just a 'readjustment rule' to clean up behind I.C. Metathesis; this is illustrated in (2)g. There is only one vowel in an imperfect stem, as in (2)h, so Truncation is not needed there, where, however, according to Brame Apocope deletes the prefix vowel.

(2)i shows the usefulness of Absolute Neutralization to bring what would otherwise be special classes of biradical verbs (e.g. ʔod 'stay') into the rules for triradicals. The absolutely neutralized consonant ʔ (voiced pharyngeal fricative) must somehow be fitted into the class 'R' (sonorant consonants), or the metathesis rule should be rewritten to refer to R + ʔ. A number of considerations converge on the choice of ʔ for the 'abstract' consonant (Brame, 51-9). The absolute neutralization rule must, of course, be ordered quite late. Nothing is said in Brame's discussion about the rule(s) needed to get from the output of Absolute Neutralization, a, to o in (2)i. Of course it is never possible to cover *everything* relevant, and perhaps Brame's analysis may be reasonably considered to have said enough.

Brame (pp. 44-5) says of the cyclic orderings in his analysis that "Stress Assignment is a cyclic rule... All other rules, with the exception of I.C. Metathesis and Truncation, are rules that apply on the final cycle... I.C. Metathesis must be either cyclic or precyclic. The rule of Truncation will also be assumed to be precyclic." Thus the analysis doesn't just rely on the cyclic principle, but also on the classification of rules into pre-cyclic (Truncation), cyclic (Stress Assignment, I.C. Metathesis), and last-cyclic (seven rules). Counting each extrinsic ordering statement as a rule (surely fair enough), and each assignment of a

rule to its cycle-class (pre-cyclic, cyclic, last-cyclic) as a rule (Not fair?), this is at least twenty-six rules.³

Now we turn to the tough problem of finding a reasonable alternative to Brame's analysis.

3. Unordered rule analysis

The no rule order principle I take to apply throughout the grammar, including to that portion of the lexical rules which provides the phonologization of a morpheme: for example, 'put' → put. The interpretation of this rule if no other rule intervenes is pronunciation according to the invariant feature specification assigned by the lexical rule, i.e., [put] (+allophonic details). Only intrinsically prior ordered other rules can intervene, and the only principle I admit for such prior ordering is *proper inclusion precedence* (Sanders 1970, Koutsoudas, Sanders and Noll 1974:8): 'A → B' is (properly) included in 'A → B / C ___', and the latter included in 'A → B / C ___ D'.

For reasons that cannot be gone into here (see Hudson 1980), but following from this strict interpretation of the no rule order principle and proper inclusion precedence, this means that morphemes which alternate (vary non-allophonically) will have their alternates indicated in the lexicon. Thus, for example, one of the subject prefixes of Maltese verbs alternates between ti and ta, with ta appearing before h/?. In Brame's analysis i → a / h/?. According to the no rule order principle this morpheme must be entered in the lexicon as:

$$(3) \text{ t } \left[\begin{array}{c} \text{V} \\ (+\text{high}) \\ (-\text{back}) \end{array} \right]$$

The interpretation of this representation is that the morpheme is ti, or tV (t + the archi-vowel).

When this morpheme is inserted before h or ? (actually, tested for insertion in the environment), the following rule, my alternative to Brame's 'Guttural Assimilation', will yield ta:

$$(4) \left[\begin{array}{c} (+\text{high}) \\ (-\text{back}) \end{array} \right] \rightarrow +\text{low} / \text{---} \left[\begin{array}{c} \text{C} \\ +\text{low} \end{array} \right]$$

This rule affects only segments lexically marked by the parenthesis as i~V. Brame's rule is expressed as a neutralization of the contrast i/a before h/?. I can find no evidence for the latter in Cohen 1970 or Borg 1975, which, however, support the alternation in these subject prefixes.

Rule (4) selects the tV alternate of (3) --since +low necessarily contradicts [+high, -back]-- and completes it as ta (since +low → -high, +back), appropriately characterizing the a quality of the vowel as an expression or sign of a following morpheme with initial h/?. Since tV is determined by a rule of the grammar, logic requires that its alternate ti occur where tV is not determined. (Some may think that a paradox will result if some other rule should require i. But if i is *required*, this is again V of (3), the archi-vowel, and the quality of V, i, is provided by the

hypothetical rule.) Thus in (3) *ti* is the 'basic' form of the morpheme, in the sense of Jakobson (1948: 156), the alternate inserted in environments where the other(s) may also occur. The basic form is the 'otherwise case'.

I am aware that this discussion may seem abstract and rushed. Extensive discussion and consideration through several examples may be necessary to make clear the implications of the no rule order principle for lexical representations and derivations from these. Even then, the criticism immediately arises: what of the vast complications of the lexicon which are implied by the parentheses on features of numerous alternating morphemes? I give three brief responses to this, and, if these are inadequate, refer the reader to Hudson 1980: 120-1.

First, the parentheses are the only non-phonetic feature material my theory will allow, or, I think, needs to allow in phonological representations. No diacritic or non-phonetic, classificatory features are allowed, and no arbitrary exception features. The introduction of the parentheses, we will see, allows considerable simplification in other parts of the grammar. We have no *a priori* way of judging the relative complexities in this trade-off. Second, we can reasonably assign parenthesized features a value *less than* that assigned non-parenthesized, invariant features; since the former are neutralized in some environment, are subject to the non-lexical grammar, they are in that sense weaker or less prominent as lexical markers. Third, the alternations of morphological form which the parentheses indicate are the raw material of grammars -- representations of the given facts of alternation. What is truly additional is the opposite assumption, that, given an alternation of allomorphs, one allomorph must (except for suppletions -- and when does phonological divergence constitute suppletion?) be chosen as *the* lexical form.

Another objection I anticipate concerns how my grammar deals with productivity, since there are no feature-changing rules. For this see Hudson 1980: 121; more could be said about the expression of productivity or extension of alternations, and as well about their non-productivity and leveling, but not here.

Where Brame assigned triradical verbs the lexical form CVCVC, I assign the form C(V)C(V)C. Where Brame assumed a rule providing the stem-shapes CVCVC for the perfect and CCVC for the imperfect, I assign stem-shape according to the more complex rule (5):

$$(5) \quad (V) \rightarrow V / \left\{ \begin{array}{l} C_{\alpha} \text{ --- } C_{\beta}(CV)_0^1 \# (\# \text{Imperf.}) \\ \#C \text{ --- } / \text{ Perf. 3rd pers.} \end{array} \right\}$$

Rule (5), which parallels Brame's unexpressed rule for the same purpose, interacts with the following phonotactic requirements of Maltese to give the right stem-shapes for Perfect and Imperfect.⁴ This is a fairly complex rule, but this is reasonably what happens when a language-stage of antepenultimate stress is succeeded by loss of unstressed vowels (which history Brame's analysis repeats). (In the following R is a member of the class of sonorant consonants.)

(6) Phonotactic constraints of Maltese⁵

- a. *CCC# c. *CRC e. *CC_αC_α g. $\acute{V}C_1$ $\left[\begin{smallmatrix} *+high \\ -back \end{smallmatrix} \right]$
 b. *#CCC d. *#RC f. *CVCVCC#

How (5) and (6) interact to give the right stem-shapes is a somewhat complex matter which I hope is sufficiently indicated in the derivations below. The rules say when a lexically given alternation, shown as '(V)', must be V, otherwise $\emptyset$ is chosen.

We will need only three other rules. As (4) corresponds roughly to Brame's Guttural Assimilation, (7)a-c correspond to others of his rules.

- (7) a. $V \rightarrow \acute{V} / _ \left\{ \begin{smallmatrix} \{V\} C \\ \{C\} C_1 \\ C_1 VC_0 \end{smallmatrix} \right\} \# \#$ Stress Assignment

- b. $\left[\begin{smallmatrix} (+high) \\ (-back) \end{smallmatrix} \right] \rightarrow \left[\begin{smallmatrix} -high \\ +back \\ -low \end{smallmatrix} \right] / \left\{ \begin{smallmatrix} (o)C_1 _ \\ [+cons] \\ -[+son] \end{smallmatrix} \right\} C_0(o)$ Vowel Harmony
 (o)

- c. $\emptyset \rightarrow (i) / \# \# _$ Prothesis

Rule (7)a assigns stress as observed in almost all Maltese words, and according to the common practice of studies of Maltese grammar: on final strong syllables, otherwise on penultimate syllables. The implication of (7)b, contra Brame, is that 'Vowel Harmony', like 'Guttural Assimilation', is lexically determined: only lexically marked alternations of $i \sim V$ harmonize with o . As with Brame's (1)h, outside the inflectional morphology of the verb there are readily found exceptions to his (1)d. Notice that (7)b conditions harmony by (o), which in my notation cannot mean 'optionally o', but ' $o \sim \emptyset$ ', 'o alternating with its absence'.

Derivations of the verbs of (2)a-i using my rules are given as (8)a-i, and are discussed below.

- (8) a. $h(a)t(a)f-[V, (+high), (-back), (-low)]t$
 $h\acute{a}tf-[V, (+high), (-back), (-low)]t$ (5)
 $h\acute{a}tf-[V, (+high), (-back), (-low)]t$ (6)b & (7)a
 $h\acute{a}tf-et$ (6)g
 b. $k(o)r(o)b-[V, (+high), (-back), (-low)]t$
 $korb-ot$ (5) & (7)b
 $k\acute{o}rb-ot$
 c. $ni-z(V)b(o)h-[V, (+high), (-back), (-low)]t$
 $n\acute{i}-zbh-ok$ (5) & (7)b
 $n\acute{i}-zbh-ok$ (7)a
 d. $n[V, (+high), (-back), (-low)]-h(V)r(a)b$
 $na-hrab$ (4) & (5)
 $n\acute{a}-h\acute{r}ab$ (7)a

- e. l(a)?(a)t-t
 l?āt-t (5) & (6)a & (6)f
 il?āt-t (7)a & (7)c
- f. ni-t(o)l(o)b-u
 ni-to**l**b-u (5) & (6)c
 ni-to**l**b-u (7)a
- g. j̣(a)r(a)r
 j̣arr (5) & (6)a/b
 j̣arr
- h. t(i)-h(o)b(V)b
 t(i)-hobb (5) & (6)a
 t-hobb (6)f & (7)a
- i. n[V, (+high), (-back), (-low)]-?ood-u
 no-?ood-u (7)a & (7)b

Discussion of derivations:

(8)a: The first stem vowel is required by (5); the second is not, so as an otherwise case it is $\emptyset$; the penultimate vowel is stressed by (7)a; (6)g makes the suffix vowel [V, -high, (-back), (-low)]; no other rule applies, so -back, -low are selected, giving e.

(8)b: This is as in (8)a, except that the possibilities of the suffix vowel [V, (+high), (-back), (-low)] are immediately resolved as [V, -high, +back, -low] by (7)b.

(8)c: Again (7)b resolves the suffix vowel possibilities as o; (5) does not require the second stem vowel, which is then $\emptyset$; nor does any rule require the first stem vowel, which is $\emptyset$; the prefix vowel is stressed by (7)a.

(8)d: (5) requires the second stem vowel but not the first; (4) requires that the possibilities of the prefix vowel be resolved as a, which is stressed by (7)a.

(8)e: The second stem vowel is not required by (5), but is by (6)a (no word-final CCC sequences); (6)f requires the absence of the first stem vowel, even though a forbidden #RC cluster results, creating an environment for (7)c, which inserts i; the final vowel is stressed by (7)a. Since the phonotactic rules require the application of (7)c in some instances, by convention of rule application we have been using it will not insert (i) otherwise.

(8)f and (8)g present no new possibilities.

(8)h shows that the vowel of the subject prefix not only alternates among i/o/a, but also may be absent, according to the requirement of (6)f, after (5) and (6)a require the first stem vowel. This possibility of alternation of $V \sim \emptyset$ in the prefix was for simplicity's sake not indicated in derivations (8)c/d/f, in which (6)f does not apply, and the vowel appears as an otherwise case. (This vowel would also be absent before V-initial stems.)

(8)i: The stem vowel is immediately stressed by (7)a, and the prefix vowel harmonizes by (7)b. The example shows that a rule like (7)b, in which an alternation is conditioned by a parenthesized (indeterminate) lexical element (in (7)b, o), applies

where the element is not parenthesized as well. This is because, again, the notation '(X)' does not mean 'optional X', but 'X~Ø', and we establish the reasonable convention, not contractible, I think, in any language, that alternations conditioned by (X) are also conditioned by X.

All the derivations of (8) have the same pattern: the possibilities of alternation are presented by the lexicon as (X). For any (X) if a rule exists requiring X but the environment of the rule is absent and precluded, the alternative to X, Ø, is selected. All the rules apply whenever and wherever their environments are fulfilled, but some alternations remain indeterminate until others are resolved. The procedure continues until all are resolved and the word is formed. In the case of one rule, (7)b, an alternation is determined by an alternation, or conditioned by possibly parenthesized material (i.e., the rule is opaque). Such rules are relatively rare and probably always subject to ongoing leveling (cf. the Ukrainian case in Hudson 1982). The foregoing analysis of the basic phonology of Maltese verbs has been proposed on rather restricted data. We need to know which of the alternations hold outside the verb morphology, and which are the products of neutralizations, or are automatic. Without more data, specifics of neither my analysis nor Brame's can be asserted with conviction.

4. Comparisons

The general aspects of the two analyses can, however, be reasonably compared, in order to judge which approach shows more promise of falsifiability, if wrong. Here are five comparisons:

(i) Brame's analysis requires in addition to a rule for stem-vocalization at least ten rules plus at least sixteen ordering statements. The alternative has four rules and no ordering statements. In fact, Brame's derivation (2)i is incomplete, and for his derivation of *nô-?ood*, not discussed here (Brame, 49), he requires additional rules. For the latter I will need a new rule, apparently a stress rule which takes proper inclusion precedence over (7)a: $V \rightarrow \hat{V} / \text{---} [\text{v CVVC}]##$.

(ii) Every rule in the unordered rule analysis including lexical representations is a true statement about Maltese pronunciations.

(iii) The unordered rule analysis by resolving some alternations by reference to the phonotactic constraints expresses these as automatic alternations. Brame's analysis makes no distinction between automatic and non-automatic alternations (or between opaque and transparent rules). The relevance of this distinction to understanding aspects of language use and language change ought to require no justification here (cf. Hudson 1980: 96-7).

(iv) The number of non-initial lines in the derivations of (2) is 28. In (8), with unordered simultaneous application of some rules, the number is 18.

(v) No principles determine Brame's analysis. Strict adherence to the no rule order principle dictates the alternative I have offered.

Campbell (1973: 8) presented Brame's rules of Guttural Assim-

lation and Vowel Harmony as evidence that "extrinsic ordering lives." Derivation of items for which Brame requires extrinsic ordering of these two rules (e.g. tó-hlóm, Brame, 33) is accomplished here with intrinsic ordering, since harmonizing vowels, which alternate with zero, are parenthesized, as shown in (8)h (not shown in (8)c/d/f), and therefore before their presence in the environment h/? is derived they undergo morphologically conditioned Vowel Harmony (7)b. The latter is a fully opaque (morphologically motivated) rule, while Guttural Assimilation is not. The latter rule, by the way, should be modified in my analysis to apply before stems with initial a (supplied by Brame with initial ?, cf. his (58)).

The system of analysis I have offered is probably not here presented entirely adequately. However, I think that readers who wished to could discover it for themselves, by working out an analysis of Brame's data and observing the principle of *no rule order*, or, equivalently, assuring that all rules and lexical entries are phonetically true statements about Maltese pronunciations.

NOTES

1. The dialect discussed by Brame seems somewhat regularized, or perhaps he has idealized it (but in non-significant ways for present purposes). See, for example, the vowel system given by Cohen 1970: 132-5.
2. There seems to be a problem with Brame's stress rule, which brackets cycles with both '#' and 'J'. His derivations omit the #-boundary, and Odden omits it from the rule (442). But without a '#' somewhere, or some identification of 'J', the rule will 'cycle out' to the sentence, or beyond.
3. According to Pullum (1979: 180) if there are *n* rules, there are *n*-factorial possible orderings of the rules. For ten rules this is 3,628,800 possibilities. The claim that rules are ordered is a wonderfully strong one, but quite untestable.
4. The expanded first case of (5), not in imperfects, is for the -CV suffixes, as in htáf-na 'we grabbed', which may be compared with hataf-na 'he grabbed us'; the former is underlying h(a)t(a)f-na#, and the latter h(a)t(a)f#na (this in answer to Odden's question, p. 444).
5. There are some properly including exceptions to these constraints such as #sCC, and #Rr where R = m (Cohen 1970: 136).

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PARAMETERS FOR A TYPOLOGY OF WORD ACCENT

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0. *Introduction.* Lockwood 1979 presented a discussion of accent systems of a type termed QUASI-AUTOMATIC. The bulk of that paper was devoted to a stratificational description of the determination of word accent in four languages often described as having automatic accent, but which do in fact have some limited phonemic contrasts in accent placement. The description used morphonic accent marks and boundary marks for exceptional placements, and allowed an automatic subsystem to deal with the regular types. One short section of the paper considered the typological aspects of this type of accent.

The present paper expands on the suggestions of the earlier work by setting forth several typological parameters applicable to word accent in general, with examples from a broader survey of data. The most important aspect of accentuation not treated here is what may be termed HIERARCHICAL ACCENT. This involves the existence of various levels of accent correlated with units of various sizes, including the superimposition of phrasal and sentence accents (or whatever one wishes to term them) upon the basic word accents. For the present, data on this phenomenon is too restricted to provide a sufficient basis for typological conclusions of much certainty and significance.

The present study is confined, then, to WORD ACCENT--the phenomenon which gives prominence to a particular syllable of a phonological word.¹

1. *Basic Distinctions.* The most fundamental division in the typology of accent systems is between AUTOMATIC accent and NON-AUTOMATIC, which could equally well be termed NON-PHONEMIC vs. PHONEMIC in the classical sense. Automatic, or non-phonemic, accent is totally predictable in terms of the phonemes, including junctures, which make up the phonological word. In any language where accent is not totally predictable in this way, accent is non-automatic or phonemic. As suggested in Lockwood 1979, the number of languages with truly automatic word accent is considerably less than a cursory review of some sources might suggest.² Still there are some genuine examples, such as the automatic initial systems of literary Czech, Finnish, or Hungarian.

Non-automatic accent can then be subdivided into TONAL and NON-TONAL TYPES. Discussion of tonal types here will be confined to defining them and giving some examples, since their typological subclassification would require more data than has been examined up to now. Even definition, however, is not completely without difficulty. Prototypical examples of tonal accent are found in Swedish, Norwegian, and Lithuanian. All these systems have two

different sorts of accentual contrast: (1) contrast of placement, just as in non-tonal systems, and (2) contrast of at least two QUALITIES, such that a phonological word may receive accent at the same point in more than one contrasting manner. In the prototypical cases cited above, the additional qualities involve pitch contours, but they can involve other phonetic dimensions instead or in addition. Two other phonetic varieties of tonal accent are in fact attested in European languages: Danish shows manifestation in terms of glottal quality (the *stød*), and standard Slovenian uses the quality of long duration.³

Non-tonal systems lack the additional dimension of accent quality, and show only contrastive positioning at the level of the phonological word.⁴ Within this type, the major subdivision is based on the morphonic (i.e. morphophonemic) determination of word accent. In the INHERENT type, all accents are determined by morphons of accent and/or by boundary markers. These morphons are assigned by the morphology, or perhaps ultimately at a still higher stratum. In the PARTLY AUTOMATIC type, however, some, and perhaps most, accents can be automatically assigned given the boundaries of phonological words (realized as a phonemic junctures), so only some accents require inherent markings. Even if they constitute only a small residue, of course, the exceptional items are sufficient to render accent phonemic. According to Paul Garde (1967, 1968) such major European languages as German, Spanish, Italian, Portuguese, and Modern Greek have inherent accent.

The partly automatic category is intended to encompass the quasi-automatic types treated in Lockwood 1979 as well as other types in which automatic determination accounts for only a relatively small number of examples. Examples of the latter variety are Russian, as described by Sullivan 1969, 1970, and the structurally very similar Bulgarian accent system, treated in part in Lockwood 1982. In terms of structure alone, the whole range from quasi-automatic to what we might term QUASI-INHERENT is indistinguishable. The difference would show up only if we applied statistical counts to a sample of the language, which would in any case give a sliding scale rather than a discrete categorization.

Figure 1 summarizes the typological subdivisions discussed so far.

AUTOMATIC (=NON-PHONEMIC)		
NON-AUTOMATIC (=PHONEMIC)	TONAL	
	NON-TONAL	PARTLY AUTOMATIC INHERENT

Figure 1.
*Basic types of
word accent.*

2. *Finer Distinctions.* At this point we have three basic types apart from tonal accent: Automatic, partly automatic, and inherent. considered according to the manner of determination used in each, these three types can be viewed as manifesting combinations of just two varieties of determination: automatic, and inherent. Partly automatic shows both, while automatic and inherent types show one only. In view of this fact, further subcategorization can be based mainly on a detailed examination of the two determination types. Only varieties of or restrictions on the combination of inherent and automatic subparts in a partly automatic system would require us to go beyond this. We can thus turn to an

examination of different varieties of automatic and inherent determination in the following subsections.

2.1 *Subtypes of Automatic Determination.* Some basic outlines for the more delicate typological classification of automatic accent determination were suggested in Lockwood 1979. The basic division presented there involved purely POSITIONAL determination, based on the number of syllables, as opposed to WEIGHTED determination, which involves syllable types of two or more different weights as one factor in determining accent placement.

Data considered in that earlier paper led to the recognition of four types of positional determination, but now this classification can be extended to six types as charted in Figure 2. The most basic distinction there is between accent which is FORE-DETERMINED (counted from the beginning of the phonological word) and that which is END-DETERMINED (counted from the end). Within each of these, we get a distinction between PERIPHERAL and NON-PERIPHERAL accent placement. Finally, within the non-peripheral category, we get, as far as known, examples of accent which is ONCE-REMOVED, or TWICE-REMOVED. The more usual terms for each of the six positions are provided at the right of the diagram.

FORE- DETERMINED	PERIPHERAL		= INITIAL
	NON-PERIPHERAL	ONCE-REMOVED	= POSTINITIAL
		TWICE-REMOVED	= POST-POSTINITIAL
END- DETERMINED	NON-PERIPHERAL	TWICE-REMOVED	= ANTEPENULTIMATE
	PERIPHERAL	ONCE-REMOVED	= PENULTIMATE
	PERIPHERAL		= ULTIMATE

Figure 2. *Types of positional accent determination.*

It should be noted that this classification applies only to phonological words of sufficient length, and a subsidiary principle applies to the residue. So in Macedonian, the basic pattern is antipenultimate, but the residue shows initial placement. Table I lists examples of languages showing each of the six accent placement patterns.

Table I. *Languages with various types of positional accent determination.*

Initial	Czech, Finnish
Post-initial	Araucanian (Echeverria and Coutreras 1965), Hopi (Kalectaca, 1978)
Post-postinitial	Winnebago (Miner 1979, 1981)
Antipenultimate	Macedonian
Penultimate	Aramaic (Arsanis 1968), Warao (Osborn 1966)
Ultimate	Oroč (Avrorin & Lebedeva 1968), Quiche (Mayers 1966)

Theoretically, of course, we might wonder about the absence of other logically possible types. While the discovery of one or two examples of still further types (e.g. POST-POST-POSTINITIAL or ANTE-ANTEPENULTIMATE) might be possible, there are rather good

practical functional reasons why we should not expect an infinite multiplication of types according to the logical possibilities, giving accent patterns based on removal four, five, or six times from the periphery. These reasons stem from the fact that phonological words with more syllables tend to be rarer than shorter ones, both within a language and cross-linguistically⁵. Unitary words with a single accent seldom have more than six or seven syllables, and in most languages a length greater than four syllables is not common. So examples which can show such a pattern would be far too few, especially in the early stages of language learning, to allow such a system to be readily transmitted between generations. Even in a language with words of greater than average length, the accentuation of shorter words is naturally more likely to affect the longer ones and serve as the basis for generalization.

If we examine various examples of weighted accent determination, we will find that they fall into two rather distinct types. One type is illustrated by Classical Arabic (Jakobson 1931:117) in which the accent is on the first heavy syllable of a phonological word, and is initial in words consisting of light syllables only. This type of system may be termed **HEAVY-ACCENTING**, since it requires the accent to be on a heavy syllable if the word contains one. The automatic subsystem of Classical Latin, on the other hand, pays attention to syllable weight only in the penultimate syllable (Lockwood 1979:217-20). A heavy syllable in such a position will carry the accent, and otherwise the antepenult will be accented. Elsewhere, syllable weight is irrelevant, and it is perfectly possible to have a word with one or more heavy unaccented syllables and an accented light syllable (e.g. *condiciō arrangement, permovēō I stir violently*). This kind of a weighted system may be termed **WEIGHT-COUNTING**. In such a system, some aspect of the distribution of weighting aids in distinguishing the application of two (or more) different positional principles, as in the Latin, where the weight of the penult distinguishes between the application of penultimate and antepenultimate accentuation.

Figure 3 charts the categorization of accent systems according to weighting.

NON-WEIGHTED	
WEIGHTED	HEAVY-ACCENTING
	WEIGHT-COUNTING

Figure 3.
Weighting and Its Subtypes.

2.2 Subtypes of Inherent Determination. Subsystems of inherent accent may be classified first of all according to the types of morphonic signals serving as input to the phonology. Here we distinguish **MORPHONIC ACCENTS** and **MORPHONIC BOUNDARIES**⁶, and a given inherent system may be either **ACCENT-MARKING** (using just accents) **BOUNDARY-MARKING** (using just boundaries) or **COMPLEX** (using both).

In the subclassification of systems employing morphonic accents, perhaps the most fundamental division to be made involves the place of realization of a morphonic accent on the phonemic level, in relation to its morphonic placement. As illustrated in

Lockwood 1979, a morphonic accent is most commonly marked on the morphonic realize of the syllable on which it will be realized phonemically. Following suggestions already made in Garde 1967, this can be termed SELF-ACCENT. In other cases, however, we may have legitimate reason to characterize a morpheme as requiring accent on a preceding or following syllable, so we can also have morphons of PRE-ACCENT and POST-ACCENT. Pre-accent is found, for example, in both Latin and Turkish (Lockwood 1979). Good examples of post-accent are found in both Russian and Bulgarian (Lockwood 1982), where some stems are regularly characterized for accent on the following syllable. Given the three potential types of inherent accent, the systems of particular languages can be classified according to which of the three types they employ. This gives us seven possible types, as summarized in Table II. When known, examples are cited, but research to date has not provided examples of all the possibilities. There seems to be no reason to expect that any of these could not occur in one language or another.

Table II. *Morphonic accent systems in relation to place of realization*

	<u>Types of Accent Morphons</u>			EXAMPLES
	SELF-ACCENT	PRE-ACCENT	POST-ACCENT	
I	+	+	+	
II	+	+	-	Macedonian
III	+	-	+	Russian
IV	-	+	+	
V	+	-	-	Italian
VI	-	+	-	Polish
VII	-	-	+	

Another important way of subclassifying languages with morphonic accents distinguishes those languages in which just one morphonic accent will be assigned to the realize of any phonological word from those in which more than one may be assigned morphonically, but only one of these will be realized phonemically. Thus we get a distinction between SINGLE and MULTIPLE accent. In the latter case, the phonological system will include some means of resolving the conflict posed by the availability of more than one morphonic accent and the requirement of only one phonemic accent per phonological word. A fairly simple example of this resolution is provided by Garde's description (1967, 1968) of the so-called 'Romance type' of accent, found in such languages as Spanish, Italian, Portuguese, and Modern Greek. In such a system, one or more syllables are marked as accentable, but only the last accentable syllable in a phonological word will actually receive an accent.

In a system of single accent, on the other hand, the assignment of main word accent is essentially completed at the morphonic level, so that each morphonic accent assigned is simply realized as a phonemic accent. This corresponds to Garde's 'Germanic' type. In that particular system, accent is placed according to the grammatical category root, so that the beginning of each root is mark-

ed as accented. Garde claims (1967:314) that this is the system in modern standard German.⁷

Multiple accenting can be relevantly subclassified according to the type of resolution which is pertinent for it. The Spanish and other 'Romance' systems, for instance, illustrate POSITIONAL resolution, in which the accent realized depends on sequence of accentable syllables in the word. Spanish specifically shows the application of the LAST-REALIZED principle, while Russian has the opposed FIRST-REALIZED principle in a subpart of its inherent sub-system, as formalized by Sullivan (1970). Other languages show STRENGTH RESOLUTION based on an abstract strength assigned to each accent in the system. Resolution in this type of system involves a stronger accent taking precedence over a weaker one. Multiple accent systems may use the positional principle alone, as in Spanish, or a combination of positional and strength resolution, which is found in Russian. Further investigation is likely to reveal types based on strength resolution alone, but no sure examples are known at the present.⁸

Figure 4 summarizes the classification of accent systems based on resolution.

SINGLE	STRENGTH RESOLUTION*	
	POSITIONAL RESOLUTION*	FIRST REALIZED
		LAST REALIZED

Figure 4.
Subcategorization of inherent accent.

*As indicated in the text, the strength and positional types may also be combined in a complex type.

The basic subclassification of boundaries was set forth in Lockwood 1979. There are PROCLITIC boundaries, which set off one or more syllables at the beginning of a phonological word as irrelevant in determining accent, and ENCLITIC boundaries setting off similarly uncounted sequences at the end. Languages using boundaries as a part of their inherent accent systems may then be subclassified according to whether they have only proclitic boundaries (Latin and modern Macedonian--cf. Lockwood 1979), only enclitic boundaries (Kabard, based on Sagirov 1967), or both (Polish--cf. Comrie 1976)⁹.

3. *Summary and Conclusion.* Table III summarizes the parameters suggested in the course of this paper, giving a name to each of them, indicating the terms associated with them, and briefly stating the structural characteristics distinguishing those terms.

Each of these structural characteristics can be associated more specifically with particulars of the phonological networks found in stratificational descriptions of accent systems. Some examples of such networks are found in Lockwood 1979, 1982. Ideally, this paper could be extended with a detailed discussion of such characteristics, but space does not permit a full treatment

Table III. *Summary of typological parameters for accent and their characteristics.*

<u>Parameter *</u>	<u>Terms</u>	<u>Structural Characteristics</u>
AUTOMATICITY	AUTOMATIC/NON-AUTOMATIC	<i>Determination on chiophonemic vs. phonemic stratum</i>
TONALITY	TONAL/NON-TONAL	<i>Presence vs. absence of qualitative contrasts restricted to accented syllables</i>
INHERENCE	INHERENT/PARTLY AUTOMATIC	<i>Absence vs. presence of determined subpart</i>
ORIENTATION	FORE-DETERMINED/END DETERMINED	<i>Determination from left vs. right</i>
PERIPHERALITY	PERIPHERAL/NON-PERIPHERAL	<i>Accent at vs. removed from periphery</i>
REMOVAL	ONCE-REMOVED/TWICE-REMOVED	<i>One vs. two syllables from periphery</i>
WEIGHTING	WEIGHTED/NON-WEIGHTED	<i>Two or more vs. one types of syllable for accent determination</i>
WEIGHT TYPE	HEAVY-ACCENTING/WEIGHT-COUNTING	<i>Obligatory accenting of a heavy syllable vs. other weighting principle</i>
MORPHON TYPE	ACCENT-MARKING/BOUNDARY-MARKING/COMPLEX	<i>Relevant morphons on syllables vs. at boundaries vs. both</i>
NUMBER	SINGLE/MULTIPLE	<i>One vs. potentially many accent morphons per accented phonological word</i>
RESOLUTION	STRENGTH-BASED/POSITIONAL/COMPLEX	<i>Accent strength vs. position vs. both relevant in resolution</i>
POSITIONAL TYPE	FIRST-REALIZED/LAST-REALIZED	<i>Actual accent on first vs. last accentable syllable</i>
BOUNDARY TYPE	PROCLITIC/ENCLITIC/COMPLEX	<i>Preceding vs. following vs. both portions set off by clitic boundaries</i>

* An additional parameter ACCENT TYPE with the seven (potential) types listed in Table II could also be added. It is omitted here because its terms are too difficult to explain succinctly.

of such matters.¹⁰

While this paper is a significant step beyond the foundations laid down in 1979 (building on earlier work by Garde and Sullivan) there is obviously further work needed to extend, refine, and correct this system. It is extremely important to extend the range of data which has been examined from this point of view.¹¹

With such an extension of the data, it should become possible to refine the typology by filling in missing examples and suggesting additional significant parameters or terms within them. Further, it is to be hoped that explanations can be found for the typological patterns which emerge. In the pursuit of explanations, it is important to proceed on the assumption that relatively few typological matters are genuinely explained by assuming they reflect innate properties of the human brain. Much more frequently, genuine explanations relate the functional realities of language to its development over history and in each individual. Sampson 1980 has sought to explain some basic universals along such lines, and the discussion of the reasons for the apparent limitations on types of positional accent determination to positions no more than three syllables from the periphery of the phonological word was done along similar lines in section 2.1 of the present paper.

N O T E S

1 This term is chosen over STRESS to avoid the phonetic connotations commonly associated with the latter. It is to be understood that accent can be manifested not only in terms of loudness or intensity, but also in terms of pitch or duration, or any combination of these.

2 Ruhlen 1975, for example, provides summaries which suggest automatic accent in a wide variety of the world's languages. A check of cited sources reveals, however, that the compilers have not separated absolutes from mere statistical tendencies, nor do they distinguish determination of accent by position in the phonological word from determination in terms of purely grammatical units such as root or stem.

3 The term QUALITY is admittedly somewhat awkward here in view of the classical opposition of quality to QUANTITY (i.e. length or duration), but it is difficult to find a better alternative.

4 Such systems commonly do have differences dependent on the hierarchical arrangement of units of different sizes and ranks, which are beyond the scope of the present investigation.

5 Some languages have greater average word lengths than others, of course. Also there are languages in which monosyllabic words in particular are rare compared to words of two or more syllables.

6 Even totally automatic accent uses boundaries to some extent, but in such systems these boundary monophons are regularly realized as phonemic junctures. So here we refer specifically to special boundaries not having this property.

7 As noted by Garde himself (1967:314) the German system is actually not as simple as Garde's discussion would have us believe. Cf. the accentual alternation in such pairs as: *Metzger* butcher; *Metzgerei* butcher shop; *Bäcker* baker; *Bäckerei* bakery.

8 Garde's treatment of Slavic languages (1967:315-316) assumes an entirely weighted system in those which do not have a completely automatic or quasi-automatic system. Sullivan 1970 and Lockwood 1982 show, however, that these systems can be treated more economically by a combination of 1) morphological determination, 2) strength-resolution on a minor scale, 3) first-realization positional resolution, and 4) automatic initial accent for a residuum.

9 Comrie 1976 provides additional data showing that Polish has both enclitic and proclitic boundaries, not just enclitic ones as claimed in Lockwood 1979.

10 Sample treatments of four accent systems (Classical Arabic, Spanish, Kabard, and Polish) were provided in an appendix to the handout for the oral presentation of this paper. Copies are available from the author on request.

11 Data examined in the preparation of this paper has been primarily from discussions of accent in individual grammars, phonological articles, and chapters of anthologies covering various languages of Europe, the Near East, the Soviet Union, and the Americas.

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IV

MORPHOLOGY AND SYNTAX

SAN JUAN SOUTHERN PAIUTE NUMERALS

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In this paper, our first object is to provide a thorough analysis of San Juan Southern Paiute numerals at the level of derivational morphology and that of syntax and inflectional morphology. Our discussion below falls into two parts based on the two levels of analysis, which involve differing sorts of data and explanation and have differing implications for the general study of numeral systems in language.

San Juan Southern Paiute is one dialect in the Chemehuevi-Southern Paiute-Ute dialect continuum, spoken from southern California through southern Nevada, northern Arizona, and southern Utah to Colorado. This continuum along with Shoshoni and Paviotso form the Numic branch of the Uto-Aztecan language family. San Juan Southern Paiute is unique among Numic languages in that it is the most important language for all generations: it is the first language of the youngest children and is spoken in all exchanges between Southern Paiutes. It is also unusual in that its speakers have not borrowed from English in adapting their vocabulary to the needs of modern life, but have used the language's own resources. As the traditional numeral system appears to have been expanded in this way as well, it provides an interesting case for the study of change in numeral systems. Both their conservatism and innovativeness are probably due to their geographic and cultural isolation from Anglo society. Their traditional lands, as described by Isabel Kelly (1976) in her general ethnography of Southern Paiutes as they were pre-contact, were already far from Anglo thoroughfares; after 1934, the lands became part of the Western Navajo Reservation, which left the San Juan surrounded by a people who spoke a quite different language and who, moreover, were traditional enemies of the Paiutes. Living on linguistic and social islands, they are even more isolated. This not so pleasant situation from the San Juan Southern Paiute viewpoint has made them a Uto-Aztecanist's paradise.

Derivational Morphology

Sapir (1930:262-66) described the derivational morphology, inflectional morphology, and syntax of Kaibab Southern Paiute numerals from one to ten, and by tens from twenty to one hundred. In this part, we will go beyond this to describe the system generating the 'in-between' numerals and the numerals from one hundred into the millions. Secondly, we will discuss some apparent limitations of the system in terms of its recent expansion and functions in current San Juan life. Finally, we add some historical

analysis of the influence of hand-counting strategies on the system.

To begin with, we give the San Juan equivalents of Sapir's Kaibab numerals in (1). All San Juan data is given in the orthography developed by Bunte and L. Jake; a Kaibab Paiute. The letters have their IPA values with the following exceptions: sh and ch have their value in English; ng is angma; u is high, back, unrounded; ø has slight r-coloring. The forms given in (1), (2), and (3) are both the counting and citation forms. The ending -yuni(a) (-yumuni(a) in the animate gender: see below) consists of the suffix, -yu-, identified by Sapir (1930:174) with 'cardinality,' and the enclitic, -ni(a), literally "like" or "approximately." This ending is optional in the numerals from one to ten; -ni(a) elsewhere in the language will be discussed in the second part. The ending, -yuni, in opposition to the oblique in -ku, marks the nominative of those numerals which agree in case with their syntactic function.

- | | | | |
|-------|------------------|-----------------|-----------------------------|
| (1) 1 | shuuyu(ni) | 9 | shuwaroxomai(yuni) |
| 2 | waay(uni) | 10 | toxomai(yuni) |
| 3 | pa(y)uni | | |
| 4 | wachui(yuni) | 20 | waamaiyuni or waamashuuyuni |
| 5 | manaxi(yuni) | 30 | paimaiyuni or paimashuuyuni |
| 6 | navai(yuni) | | |
| 7 | navaikyava(yuni) | 40 (50, ... 90) | wachui (manaxi, |
| 8 | waawachui(yuni) | ... | shuwaroxomai) toxomaiyuni |

The numerals from one to ten were unanalyzable units to our informants; their historical derivation will be discussed later in this section. Twenty to ninety are enumerations of groups of ten. The root for ten in twenty and thirty is mai or mashuu and is related to the instrumental prefix, ma- "with the hand." The Kaibab version of mashuu, mashungwi, is analyzed by Sapir (1930:263) as hand-one=plural or "pair of hands." Forty and on use toxomai, the modern word for ten with the prefix, toxo- "exactly." 'In-between' numerals, those between the multiples of ten, use a construction with the numeral representing the 'ones' as the apparent object of a verbal form, sipikingwaaxat, literally "there are (so many) coming along in." The numeral is always in the oblique case and the verb, a participle which can bear case, is in the nominative, as it must originally have been a main verb. Numerals from eleven to nineteen simply have this expression without the understood ten; those above twenty have what appears to be a postpositional phrase enumerating the tens first:

- (2) 11 shuu-ku sipi-ki-ngwaa-xa-t(y)
 one-obl. enter-come along-more-have/be-participle=nom.
- 25 waamai-yu-kwai-ni manaxi-ku sipi-ki-ngwaa-xa-t
 twenty-card.-beyond?-like five-obl. are more coming in
- 53 manaxiyukwaini paiky sipikingwaaxat

This system was checked with Kaibab Paiutes and is virtually identical to their usage. However, none appeared familiar with the system in San Juan for the larger numerals. In San Juan Southern Paiute, the numerals from 100 to 1,000 are constructed along similar lines to those above, with the difference that a new term is introduced, *mɔɔ* "hundred." *Mɔɔ* as will be seen below is clearly related to the gestural numerals for hundreds. This form displays several peculiarities. First, the enumerating numeral of *mɔɔ* is always oblique regardless of syntactic function. *Mɔɔ* may be oblique, too, but it, like *paa* "water" and all one-syllable words with word-final, geminate vowels, is not susceptible to the phonological rule which devoiced and optionally deletes final vowels (see Franklin and Bunte (1980)). As the oblique case-ending, *-a*, is a final vowel deleted by the rule when it occurs, whether or not the stem-final vowel is devoiced or deleted itself becomes the only way of determining case for personal pronouns, demonstrative pronouns, nouns, and participles. Another peculiarity of *mɔɔ* is that it has an animate gender form with what appears to be an inserted participial marker, *-tu*, *mɔɔtʉmɔni*.

- (3) 100-900 *shuuky* (or *waaky*, etc.) *mɔɔ*
one-obl. (or two-obl., etc.) hundred-?

- 131 *shuu-ky mɔɔ-xwai-ni paimai-ky shuu-ky sipikingwaaxat*
one-obl. hundred-beyond?-like thirty-obl. one-obl. are
more coming along in

Above this point, European languages introduce lexemes expressing ever larger groups: *thousand*, *million*, *billion*, etc.; large complex numerals take the form of a list of totals, starting with the numbers of the largest group, then the next largest, and so forth. Thus, a large numeral is given as so many millions, so many thousands, then hundreds, after these come forms historically derived from enumerations of tens, or perhaps tens and twenties, and finally come simple numerals that implicitly enumerate ones. The numerals in San Juan Paiute thus far also exemplify the listing strategy. However, with 1,000 and above there is a preference for expressing the numeral as an enumeration of tens, even when it is quite large. The introduction of a new term, *avaata mɔɔ* "big hundred," does little to affect the preferred numeral strategy:

- (4) 1,000 *shuuky mɔɔ toxomaiyuni* (100 x 10)
10,000 *toxomaiy mɔɔ toxomaiyuni* (10 x 100 x 10)
25,000 *waamaiyukwaini manukiku sipikingwaaxat mɔɔ toxomaiyuni*
ni (25 x 100 x 10)
100,000 *avaata shuuky mɔɔ* ("big 100")
or *toxomaiy mɔɔ mɔɔ toxomaiyuni* (10 x 100 x 100 x 10)
1,000,000 *avaata mɔɔ toxomaiyuni* (100,000 x 10)
17,000,000 *avaata navaikyavaky sipikingwaaxat mɔɔ toxomaiyuni*
(1700,000 x 10)

This system, which compared to English is somewhat more explicitly a base-10 system, generates very large round numerals

easily. However, to our knowledge it is never used to generate very large complex numerals (e.g. 251,452) and there exist no conventions for expressing such numbers in San Juan Southern Paiute. Rather than belabor this point, let us pass on to a discussion of its significance for the historical development of the system.

In his book, *Africa Counts*, Zaslavsky (1973:30) states: "The development of numeration in any society ultimately rests upon the economic development of the society." Within the lifetimes of the eldest members of the community, the San Juan have seen their economic life pass from hunting, gathering, horticulture, and small-scale trading to sheep and cattle herding, occasional wage labor, and production of goods for trade on a relatively large scale. Moreover, as the San Juan become increasingly involved in the political and economic affairs of the world beyond their tribe, even larger figures than those occurring in their economic practice come up in their discussions of these affairs. The numeral system as it exists above the tens of thousands at least appears to have been grafted onto the previous well-developed system for smaller numerals in the last thirty or forty years. Most older informants do not use, and seem to have great difficulty interpreting, numerals above the approximate threshold of 10,000. Those that use these large numerals, use them 'passively' as they tend to be figures cited originally in newsprograms or other outside sources. They are not used in their own economic practice. This perhaps explains why the system has conventional expressions only for very large round numbers. However, this system has obvious possibilities inherent in it that may yet be developed if there is a need. We would predict that the system would be remodeled according to a listing pattern using avaata mɔɔ (100,000s), mɔɔ (100s), and toxomai (10s) and that the preference for decimality in large numerals would be abandoned for the sake of efficiency and perspicuousness in the complex numerals. As a parting note on this topic, we note the interesting parallel between the Romance derivation of million "big thousand" from Latin mille and the San Juan creation avaata mɔɔ "big hundred" from mɔɔ.

Our final topic of discussion under derivational morphology concerns the influence of hand-counting strategies on the structure and lexemic elements of the San Juan numeral system. Zaslavsky (1973) describes two dominant hand-counting strategies in Africa for the numbers from one to ten. One, the quinary or base-5 system, constructs the the gesture-numerals by building up to five on one hand and then adding fingers from the other to construct six to ten. Like our own, the current San Juan system for counting one through ten is of this type. One through five are counted on the left hand; the right hand tucks the little finger and then the others into the palm until the left hand is a fist. Six to ten are counted on the right hand by the left in similar fashion except that the right hand is held palm down. This procedure is repeated for each series of ten when counting by ones. There are special procedures for counting by twos and threes used in the distribution of goods and money which differ somewhat from counting by ones. The other African hand-counting strategy described

by Zaslavsky is "the method of finger-reckoning called 'representation by two approximately equal terms'" (1973:48). In this system, the numbers from one to ten are reckoned with both hands, each holding up equal numbers of fingers for even numbers and approximately equal numbers for odd numbers.

The linguistic numerals of African peoples using either hand-counting strategy, according to Zaslavsky (1973:46-47), show an "intimate relationship between number words and finger counting." The historical derivation of the root for ten, mai "hand(s)" or mashuu "pair of hands," and perhaps that of manaxi "five" not only speaks for a relation between linguistic and gestural numerals; it suggests that the hand-counting system influencing the choice of terms for multiples of five was at least partially a quinary one. However, the names for the numbers four, six, seven, and eight, which are wachui, navai, navaikyava, and waawachui, are quite similar in structure, when analyzed historically, to numerals found in African systems dominated by 'approximate equals.' Wachui is wachangwi in Kaibab; or, on the model of the alternate root for ten, mashungwi "pair of hands," "pair of twos." Navai, as Sapir suggests (1930:263), appears to be constructed of na-, the reflexive suffix, and pai "three" and perhaps meant "three on itself." Although navaikyava "seven" is clearly based on navai, the nature of the connection is obscure. On the other hand, waawachui "eight" is transparently "two fours" or even "two pairs of twos." If Southern Paiutes used a hand-counting system based on 'approximate equals,' it was quite a long time ago. All dialects in the Chemehuevi-Southern Paiute-Ute continuum have equivalent terms; moreover, Sapir notes (1930:263) like terms are found in Hopi and Nahuatl, Uto-Aztec languages distant from Southern Paiute by thousands of years. In any case, although informants recognized when it was pointed out to them that these numerals appear to denote pairs of equal numbers, none could remember having ever heard of Southern Paiutes of any tribe ever using a related gestural system.

We save for last the case of moo "hundred," which exactly mirrors the present-day hand-counting convention for hundreds, and the expression, sipikingwaaxat, about which we are less sure. Moo translates literally as "finger." When counting or signalling hundreds, one or both hands are held with the palms toward the interlocutor. Fingers are held up in an order that closely resembles the Anglo counting system for ones: first, the index finger is held up, then the middle finger, the ring-finger, the little finger, and finally the thumb. When one hand is complete, the other is used. Holding fingers up as we do for signalling ones by convention only means hundreds among the San Juan. This counting procedure, in which the fingers stand out as they do not in the other procedures described above, probably lies behind the adoption of moo in San Juan Southern Paiute to refer to hundreds. That moo and its enumerating numeral appear in the oblique whether or not the numeral stands in subject position suggests that the expression may be a now lexicalized elliptical reference to hand-counting from which the verb governing moo is now missing. This

would make it similar to the construction with the verb, sipikingwaaxat, to which we now turn. It is also likely that this expression was at one time intended to describe the hand-counting process as it continues to well describe the tucking-in procedure of today. However, none of our San Juan informants were aware of the significance of the motion-verb part of the verbal construction, sipiki- "come along in," despite the fact that it is used in other, everyday contexts. Moreover, in Kaibab Southern Paiute, which shares the sipikingwaaxat construction, the verb sipi means "emerge, appear," not "enter," and is so glossed in Sapir's Kaibab dictionary (1930:700). This seems more appropriate to a counting system like our own where the fingers 'emerge' or 'appear' from a closed fist. The presence of the motion verb element, whatever its original meaning, suggests a reference to gestures; nonetheless, the expression no doubt long predates the present hand-counting system.

Syntax and Inflectional Morphology

In his (1978) article, "Universals in the Syntax of Numerals," G. G. Corbett argues for two universals in the syntactic and inflectional behavior of simple, or mono-lexemic, numerals: first, that their behavior places them between the categories, adjective and noun; and, second, if there is inconsistency in the behavior of the numerals of a language, then the smaller numerals will pattern after adjectives and the larger, after nouns. In this section, we argue that San Juan Southern Paiute numerals fall between adjectives and pronouns and have no noun-like behavior.

The simple numerals of San Juan are those in (1) plus the forms ending in mɔ and possibly avaatu mɔ. Although the others are irrelevant for Corbett's claims, we discuss them anyway for their own sake, not as counter-evidence. The simple forms in (1) and all complex forms ending in toxomaiyuni, e.g., those in (4), are marked for case, using -yuni/-ku, according to the noun they modify or their own function in the sentence if they stand alone. Forms in sipikingwaaxat and (avaatu) mɔ are invariable for case. The forms in (1), those ending in toxomaiyuni, and those in (avaatu) mɔ bear the animate suffix, -ma, historically related to the noun plural, -ma/-ngwa, when the referent is an animal or human being; forms in sipikingwaaxat do not. No numeral carries a plural marker. Any numeral may occur before the noun, between a demonstrative and any adjective; directly after the noun; or all by itself with no modified noun. They may even occur outside of the NP with a modified noun. The following examples illustrate these points:

- (5) Shuu-ku mɔ-tu-ma-ni kavangwa panikya na'
One-obl. hundred-anim. horse-pl.-obl. see I
I see a hundred horses.
- (6) Kavangwa shuu-ku mɔ-tu-ma-ni panikya na'
horse-pl.-obl. one-obl. hundred-anim. see I
I see a hundred horses.

- (7) Shuu-ky ~~moo~~-tu-ma-ni waan wangwi-y
 one-obl. hundred-anim. over there stand=pl.-pres.
 There's a hundred horses standing over there.
- (8) Waa-ky moa-xa-t
 Two-obl. father-have/be-part.=nom.
 (She) has two fathers.
- (9) Tangw'ats pichi-puxa shuu-yu-ma-ni
 Man-nom. arrive-past one-nom.-anim.
 One man arrived.

From the viewpoint of inflection, we have already seen that those numerals that mark for case do so with a different declension than do nouns. With regard to Corbett's second universal, it is interesting to note that, of the simple numerals, those in (1) plus ~~moo~~, it is only the smaller that carry case and are thus more noun-like than the larger. All of the simple forms are marked for animacy using -ma, and are thus similar to the adjectival forms we shall see below and differ from nouns. The complex numerals in sipikingwaaxat and forms in ~~moo~~, being derived historically from nouns, might be considered more noun-like than the smaller numerals in (1), fitting Corbett's claim; however, their case-marking is so idiosyncratic and unlike the norm for nouns, that the resemblance is best considered an historical accident. From our discussion of pronouns, adjectives, and participles below, it will be clear that the syntax of numerals bears no resemblance to that of nouns that it does not bear to that of other categories. Nonetheless, to be clear, we would point out that no numeral occurs in anything like a noun-noun construction with its head noun: those forms which carry case agree with the noun; with those that do not, the noun still carries the case required by its sentential function as subject or object. There are no partitive-like constructions in San Juan like those in other languages with the larger numerals.

The other categories to which we may compare numerals are adjectives and pronouns. There are two candidates for the category of adjective in San Juan. The first class is that which we call true adjectives. These all end in -ni(a), which may be taken as a similarity to numerals. When attributive, these forms invariably precede the noun they modify; they also occur as predicate adjectives and as adverbs. They do not mark for case or number but insert the animate -ngwa-, again related to the noun plural, between the stem and -ni(a). Examples (11)-(13) illustrate:

- (11) Tu-ngwa-nia-tuka
 fast-anim.-like-you=sing.=nom. eat
 Hurry up and eat.
- (12) Axava-'a' pa-ni yungapi wachu-ka
 where-you=sing.=nom. pretty basket-obl. put-perf.
 Where did you put the pretty basket?
- (13) Mang a' pa-ngwa-ni na'intsits waan kara-y
 that=anim-nom. pretty-anim. girl-nom. over there sit-pres.

The pretty girl is sitting way over there.

The other candidate for adjective is the participle in -ta. These, as was stated before, are nominalized verbs. They typically bear case in the manner of nouns; however, when they have an animate referent, they optionally take the marker, -ma, like numerals. Moreover, when carrying -ma they are not marked for case, making them in that way like true adjectives. There are a host of stems, denoting qualities like color and size, that occur only participialized. Unlike adjectives, participles may occur before or after the noun. Consider the following examples:

- (14) Axani-ang piiɬtsi pitia-ra-m yangwi'mia-vani
how-3=anim=sing baby-obl. heavy-part.-anim. carry-future
How are (you) going to carry that heavy baby?
- (15) Axa-ra'-ru kumwi-ni-xya
how-be-part.-obl. corn-own-have/be
What kind of corn do (you) have?
- (16) Shana-k~~oo~~-xwa-t mama'uts panamarupu-y
Pine pitch-chew-have/be-part.-nom. woman-nom. roll bread-pres
That gum-chewing woman is rolling out the dough.

The final category to compare numerals with is that of pronouns. As demonstrative pronouns are the ones which may modify nouns, we will examine their behavior. Demonstrative pronouns carry case of the same declensional type as nouns and participles; unlike participles and like nouns, this is obligatory. Like nouns and unlike participles or true adjectives, animacy or inanimacy is inherent in the word and is not marked. When modifying a noun, a demonstrative pronoun occurs at the beginning of the NP, after, or even elsewhere in the sentence, much like numerals. However, there is no restriction on the number of times a demonstrative may occur coreferring with a noun or itself. Pronouns, like numerals and to a lesser extent participles, may also occur independently, functioning then much like a personal pronoun. Example (17), in addition to the foregoing examples, should illustrate the possibilities:

- (17) Mang tangw'ats/tangw'ats mang pichi-vya-ang (mang)
that=anim.-sing.-nom. man-nom. arrive-future-3=sing=anim.
That man is going to come.

From the preceding discussion, it should be clear that, from the viewpoint of inflection, the simple numerals have characteristics of true adjectives, participles, and pronouns; they appear closest to participles in this regard. First, except for forms in moo, they bear case unlike true adjectives. Second, all simple numerals use the animate marker, -ma, unlike true adjectives which use -ngwa, or pronouns which use -ma/-ngwa as a plural marker. Syntactically, although it appears they have a slot of their own in the NP in pre-noun position, between the demonstrative and adjectival modifiers, their syntax generally is that of pronouns. We may say then that numerals lie between adjectives and pronouns

and not between adjectives and nouns. Moreover, there is no clear correlation between the size of simple numerals and their 'pronouniness' or 'adjectivalness.' We end this section with a listing of other forms that share some or all of the traits of numerals. Hang-, in (18), is the most like the numerals of (1); it shares all of their inflectional pattern and syntax. Mang- lacks the cardinal suffix, -yu-, and appears not to modify nouns within the NP. Kuma- and axavai- are perhaps the most interesting as they display a morphology that partakes of numeral patterns as well as pronominal or adjectival patterns. The oblique of kuma-, in -ku, is of course a numeral pattern as is the inanimate nominative, kumashu: the latter is a Kaibab numeral nominative pattern. Kumang is strictly pronominal. As may be seen in (22), axavai- has adjectival, participial, and numeral forms, all with different meanings. We propose to categorize these forms and numerals as quantifier pronouns.

- (18) Hang-yu-ma-ni kuchupuku-ngw waan wangwi-y
How many-anim.-nom. cows-pl.-nom. over there stand=pl.-pres.
How many cows are there over there?
- (19) Na' mang-ky tyka-ngu-kwa-ak
I all-obl. eat-momentaneous-perf.-it
I ate it all.
- (20) Kumang/kumash kara-y uva
Other-anim.-nom./other-inanim.-nom. sit-pres. there
Another one/more of it is sitting up there.
- (21) Kumaky a'onti'i-ya-n
Other-obl. want-pres-I/me
I need more.
- (22) Axavai-tu/axavai-ni(a) axavai-ky mani-kya-n
How big-part.-obl./how big-like/how big-obl. do-pres./past-I
How big of a one/how much/how much do I use?

Conclusion

In this paper, we hoped to provide a complete description and analysis of San Juan Southern Paiute numerals from the linguistic standpoint. In doing so, we may have strayed somewhat into the domain of anthropology. We hope that our approach, especially in the first section, turned over some new stones in the study of numerals in Native American languages. We want to emphasize the importance of observing the language-user in his or her natural habitat, as it were, and the importance of looking at language not just from the theoretical assumption that there is internal systematicity but also from the assumption that the use people put language to in the present and over history shapes it, sometimes with foreseen consequences sometimes not.

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STATISTICAL EVALUATION OF MORPHOSYNTACTIC VARIATION:
EVIDENCE FROM PORTUGUESE AND SPANISH.

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at Baton Rouge

The primary motivation for the present study is threefold: (1) to illustrate the value and restrictions of chi-square and analysis of variance for evaluating morphosyntactic variation; (2) to demonstrate hitherto unknown significant differences in past participle variation in Brazilian Portuguese; and (3) to demonstrate previously unknown sociolinguistic correlates of relative pronoun variation in Spanish.

Although phonology provided the first testing ground for theories of linguistic variability (Cedergren 1973), the variable rules introduced by Labov (1969) are certainly applicable at higher levels of grammar, as illustrated by Gillian Sankoff (1973). During the seventies, statistical evaluation of linguistic variability became quite sophisticated as can be seen by the introduction of the multiplicative model (Cedergren and David Sankoff 1974) and multidimensional scaling (David Sankoff and Cedergren 1976). Nevertheless, elementary statistical models, such as chi-square and analysis of variance, prove to be valuable tools for the evaluation of morphosyntactic variation.

To illustrate the value of the chi-square statistic for analyzing morphosyntactic variation, it was used in an experimental design made to determine the probabilistic tendencies of selection of past participles in Brazilian Portuguese (Powers 1982).

In Portuguese, there are a number of past participles that have two different forms. One follows the regular morphological paradigm ending in -do, while the other one ends with an alternate form.

A review of the literature that deals with this variation indicates conflicting generalizations, usually categorical in nature. The most common generalization, made by both American and Brazilian grammarians, is that the regular (-do) form is used in the active voice (after ter), while the irregular form is employed after either ser (passive voice) or estar (as an adjective). Silveira Bueno (1968) exemplifies this common viewpoint. Twenty years before this generalization was made by Silveira Bueno, Pereira (1948) correctly observed that many irregular forms are still used in the perfect tenses. Nevertheless, his observations were categorical in nature, which proves more often than not to be inaccurate. The study done by Said Ali (1965) has to be considered among the most extensive to date that deals with past participle variation in Brazilian Portuguese. Unlike most previous treatments or later ones that categorically state that either one form or the other is selected in a given linguistic environment,

he wrote that one form is preferred over another. Although worthy of credit for his noncategorical treatment of this variation, Said Ali fails to objectively test the probabilistic selection of one form over the other through statistical evaluation.

The treatment in the present study, that is, statistical evaluation of past participle variation in Brazilian Portuguese, attempts to provide tendencies of selection according to linguistic environments based on accepted probabilistic principles. A written questionnaire was designed in which native Portuguese speaking Brazilians completed a total of ninety-one sentences with either the regular or the irregular form of the corresponding past participle which preceded the blank in infinitival form between parentheses. A total of ten Brazilian informants made up the sample population. The frequency of each response was tabulated and the expected value was always five. Although the degrees of freedom equalled one, the correction for continuity was not used since this has been proved statistically unnecessary (Young 1980). All scores were divided according to the verb and according to the linguistic environment: perfect tenses (following ter) passive voice (following ser), and as an adjective (usually following estar). The same verbs were scored independently when semantically different: eleger (to elect +HUMAN; to elect -HUMAN); suspender (postpone; hang). After performing the chi-square analyses on the data, a table was consulted in order to determine if each result was large enough to reject the null hypothesis that no difference in usage exists.

In the present study, a total of twenty-six verbs were tested in the three linguistic environments. Of these, results only support the most common generalizations that the regular form is selected after ter and the irregular form elsewhere in twelve (46%) and eleven (42%) verbs respectively. These verbs can be seen in Table 1 and Table 2.

morror	enxugar	pegar	limpar
matar	encher	defender	misturar
acender	ocultar	envolver	sacar

Table 1: Regular form (-do) of the past participle after the auxiliary verb ter.

morror	eleger (+HUMAN)	aceitar
entregar	expulsar	prender
soltar	suspender (postpone)	salvar
matar	eleger (-HUMAN)	

Table 2: Irregular form of the past participle after the auxiliary verbs ser and estar.

Another common generalization, made by all known texts, is that only the irregular form of the past participle is currently used with the four verbs, escrever, cobrir, fazer and abrir.

Since all grammarians agreed that there is no variation among these verbs, each one was only tested in one environment. Much to our surprise, in the active voice, after ter, both abrido and aberto were selected.

Without examining every statement made by different grammarians regarding selection of individual verbs in specific environments, it is possible to reclassify the different combinations of the twenty-six past participles tested in all three linguistic environments according to their regularity and environment. The results can be seen in Table 3.

1. Regular in all environments: defender, misturar.
2. Irregular in all environments: eleger (+HUMAN)
3. Either in all environments: imprimir, extinguir.
4. Regular in the perfect tenses, irregular elsewhere: morrer, entregar, soltar, matar, expulsar.
5. Either in the perfect tenses, either in the passive voice, irregular as an adjective: expressar, fritar, submergir.
6. Either in the perfect tenses, irregular elsewhere: eleger (-HUMAN), salvar, suspender (postpone).
7. Regular in the perfect tenses, either in the passive voice, irregular as an adjective: acender, enxugar, ocultar, limpar, secar.
8. Regular in the perfect tenses, regular in the passive voice, irregular as an adjective: pegar, encher.
9. Regular in the perfect tenses, regular as an adjective, irregular in the passive voice: envolver.

Table 3: Selection of either regular (-do) or irregular (not -do) past participle forms in perfect tenses (following ter), passive voice (following ser), or as an adjective not in the passive voice (usually following estar).

Although the findings successfully reclassify past participle variation in Brazilian Portuguese based on the chi-square statistic, there remain many questions unanswered. This study fails to take into account both the geographical and social differences that we usually find. Therefore, it is suggested that these results be further examined in one geographic area of Brazil dealing with social constraints (i.e., age, education, sex) in addition to linguistic constraints.

An example of the application of analysis of variance to morphosyntactic variation can be seen by examining its use in correlating relative pronoun selection in Spanish with the formal written register of native speakers from Mexico City with different linguistic environments and with the social parameters of education, sex and age (Powers 1981).

In Spanish there are at least sixteen different linguistic

environments in which relative pronouns can be found. Variation was found in every environment.

A review of previous literature shows that at least fourteen well-known studies have been dedicated to relative pronouns in Spanish in the twentieth century, excluding the many competing prescriptive grammars. Kursteiner (1904) did a minute study of variation in one novel written between 1378 and 1403. Jones (1948) gave many prescriptive rules but never identified his sources or methodology. Fernández Ramírez (1951) consulted heterogeneous literary works written in the nineteenth and twentieth centuries in order to write a descriptive grammar of mid-twentieth century common Spanish as spoken by learned and university educated speakers from Madrid. Sullivan (1953) studied the variation of que, el que and el cual after prepositions with a nonhuman antecedent by consulting 139 volumes of science fiction. His methodology is not statistically sound because he never did an accurate count on the relative pronouns that were selected. Alarcos Llorach (1963) attacked the traditional binary classification of que as a relative pronoun versus que as a conjunction. He also noted that selection between el que and quien(es) is stylistic although he didn't define the difference between one style and the other. Cressey has written three different studies that touch upon relative pronoun variation in Spanish (1966, 1968, 1970). The first is an analysis presented within the framework of generative-transformational grammar as developed by Noam Chomsky (1965). The second study (1968) is an article to show that adverb clauses can be analyzed as transformations of relative clauses. And the third study (1970), is a transformational analysis of the relationship that exists between the relative clause and indirect questions in Spanish. Isolated comments concerning variation of relative pronouns are made in all three studies, but no quantitative measurements are given. Hernández has written two articles that deal with relative pronouns (1970, 1973). In his first study, he treats the binary classification of que as relative pronoun versus conjunction. In the second article, he attempts to reorganize with functional criteria the morphemes of relation, union and subordination. Montes Giraldo (1972) wrote after examining diverse Colombian publications, that Colombians tend to hypercorrect by avoiding selection of the stigmatized relative pronoun que. Roldán (1973) studied the structure of Spanish articles with pronouns using Postal's (1966) model for English. Plann (1975) noted that it is acceptable for an appositive relative clause to begin with either an article plus cual or an article plus que. She also wrote about other variations, although her explanations are never explained in terms of probability. Ozete (1981) designed an experiment to examine variation of relative pronouns by analyzing data from newspapers and magazines from Spain, Argentina and Mexico published from February to April, 1977. A total of 785 relative pronouns were examined and results were described in terms of percentages.

The findings of the present study attempt to go beyond all previous studies in scope and methodology. Unlike all previous

studies which treat from one to three linguistic environments, the present one subcategorizes alternation into sixteen linguistic environments. In addition to greater specification of linguistic environments, correlations with social parameters of sex, age and education are made.

Sample population and methodology. Linguistic data were gathered in May, 1980 from 292 informants at two universities, government offices, two neighborhoods, and a number of stores and restaurants in Mexico City. Informants ranged between the ages of 14 and 65. One-hundred and sixty of the questionnaires distributed and collected were analyzed because the informants answered more than 95% of the responses of the questionnaire in both linguistic sections.

Informants were divided into three educational levels, both sexes and two age levels.

Random selection was assured by stopping every second person, or at every second store or kiosk.

The testing instrument consisted of two different approaches: sentence completion test and sentence combination. A total of 8,800 choices were analyzed in the sentence completion test, while 6,880 sentence combinations were made in the second part of the test.

All subjects were tested individually, although sometimes a number of individuals were tested simultaneously always assuring independence of the others' answers. They were tested where I found them.

After establishing the fact that the informant had lived in Mexico City for the last five years, the section eliciting biographical data, its importance, and the two linguistic sections, were explained.

Each response was scored for the presence of a certain relative pronoun or a certain group of relative pronouns. Responses from the two linguistic sections were scored independently.

An analysis of variance was performed on the percentages and raw scores respectively using the ANOVA 8 program, Marston's program for analysis of variance for unequal subjects per group. The test for significant differences within all types of constructions across generations, sexes, and educational levels employed a factorial design (3x2x2) where the between-subject variables were educational level (3 levels), sex (2 levels), and generation (2 levels), and syntactic-semantic construction (1 level) was the within-subject variable. Separate tests were performed for each syntactic-semantic construction across between-subject variables. When the same relative pronoun or group of relative pronouns was expected, syntactic-semantic constructions were tested together across between-subject variables.

Results. The social parameter based on education proved highly significant in eight cases, significant in four, and marginally significant in one (Table 4).

prepositional phrases - <u>el cual/el que</u> :	$p < 0.001$
relative possessives - <u>cuyo/de quien</u> :	$p < 0.001$
nonrestrictive clauses with human antecedents: <u>quien(es)/el cual/el que</u> :	$p < 0.001$
relative clause as subject after <u>ser</u> with agreement of the verb in the clause - <u>quien</u> :	$p < 0.001$
implied human antecedents - <u>quien(es)</u> :	$p = 0.004$
nonadjacent relative pronouns - <u>el cual/el que</u> :	$p = 0.006$
prepositions with more than one syllable - <u>el cual</u> versus <u>el que</u> :	$p = 0.006$
reduplication of a clause - <u>lo cual/lo que</u> :	$p = 0.013$
nonadjacent relative pronouns - <u>el cual/el que</u> :	$p = 0.020$
monosyllabic prepositions plus <u>que</u> that cannot be confused with adverbial conjunctions - <u>el cual</u> :	$p = 0.046$
monosyllabic prepositions which when followed by <u>que</u> might be confused with adverbial conjunctions - <u>el cual/el que</u> versus <u>que</u> :	$p = 0.052$
relative clause as subject after <u>ser</u> without agreement of the verb in the clause - <u>quien(es)</u> :	$p = 0.090$

Table 4: Linguistic environments and relative pronoun selection that revealed highly significant to marginally significant differences based on educational levels.

Relative pronoun selection based on sex differences revealed highly significant differences in one case, significant differences in two cases, and marginally significant differences in one case (Table 5).

nonrestrictive clauses with nonhuman antecedents - <u>que</u> :	p = 0.005
nonrestrictive clauses with nonhuman antecedents - <u>que/el cual</u> :	p = 0.029
relative possessives - <u>cuyo/de quien</u> :	p = 0.043
relative clauses - <u>que</u> :	p = 0.091

Table 5: Linguistic environments and relative pronoun selection that revealed highly significant to marginally significant differences based on sex.

Generational differences stratified relative pronoun selection significantly in three environments and marginally in one (Table 6).

monosyllabic prepositions plus <u>que</u> that cannot be confused with adverbial conjunctions - <u>el cual/el que</u> :	p = 0.016
relative adverbs - <u>cuando</u> versus other relative adverbs:	p = 0.023
monosyllabic prepositions which when followed by <u>que</u> might be confused with adverbial conjunctions - <u>que</u> :	p = 0.031
relative adverbs - <u>cuando/donde/como</u> :	p = 0.070

Table 6: Linguistic environments and relative pronoun selection that revealed significant to marginally significant differences based on generational differences.

In conclusion, it should be clear that both chi-square and analysis of variance can prove to be very powerful tools for evaluation of morphosyntactic variation, thus lending objectivity to what otherwise might be misunderstood as free variation. In addition, since probability takes into account the size of the sample population, a p-value is much more meaningful than a mere percentage.

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CLAUSE AND SENTENCE AS DISTINCT UNITS IN THE MORPHOSYNTACTIC
ANALYSIS OF ENGLISH AND THEIR RELATION TO SEMOLOGICAL PROPOSITIONS
AND PREDICATIONS

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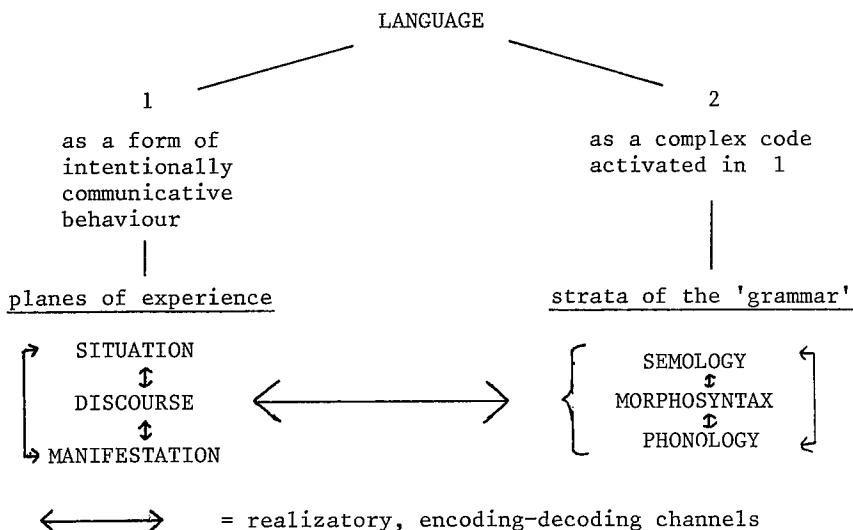
I am working, with Karen Malcolm, on a development of systematic functional linguistics in the light of insights from other schools of linguistics, particularly Pike's and Longacre's tagmemics and Fleming's stratified communication model, a development we ambitiously call 'communication' linguistics. Discourse, which we regard as the alpha and omega of linguistic description, is viewed as the linguistic exchange of messages, a central kind of communicative event. Consequently, we attempt to hold a balance between encoding and decoding perspectives, because there has been, I suggest, an over-emphasis within systemics in recent years on the paradigmatic perspective which, together with the laudable desire to test statements by arranging them canonically and productively, has led to an increasingly encoding perspective. As a result there seems to have been a tendency to under-emphasize the general categories of unit and class and, to an extent, structure which is relegated to a realizatory rôle, while system and system networks have become all. But language in the communicative event does move from then to now; it has an horizontal dimension. System is, of course, a good way of characterizing the choices the encoder makes, but the decoder meets these choices realized in chains of units. So this paper will pay attention to unit and structure, recognizing the importance of Pike's particle perspective, and Beekman and Callow's and Fleming's awareness of the 'packaging' principle in language (cf. Pike 1967, Pike & Pike 1977, Beekman & Callow 1974, Beekman, Callow & Kopesec 1981, Fleming 1974, 1978).

There has also, over the last ten years, been a lack of clarity amongst some systemacists as to which 'level' or 'stratum' particular statements belong (cf. Butler 1979, and Fawcett 1980, for discussion of this tendency in Halliday's work). Here an attempt will be made to be explicit about the stratum which is the locus of the statements being made.

Language can be viewed both as a form of intentionally communicative behaviour and as the complex code which such behaviour activates. This is not a competence-performance dichotomy but a code and behaviour perspective (cf. Halliday in press). We recognize both planes of experience in the behaviour and strata in the realizatory code cycle. The three planes of experience in the linguistic communicative event are situation, discourse, and manifestation. Discourse is the linguistic realization of what has been encoded as communicatively relevant for

the decoding receiver(s) by a language user or users. The real or imaginary incidents, relations, animate and inanimate objects, intents, attitudes etc. that are selected and those that might well have been selected but have not been, i.e. those that are 'at risk', comprise the situation of discourse. Manifestation refers to the substance (phonic or graphic) which transmits what is being communicated. It is the physical realization of choices from the code which have been made in order to create discourse in situation. Three strata of language as code are recognized: semology, morphosyntax, and phonology. Adapting Halliday (1979) we say that 'meaning' choices at the semological stratum are realized by 'wording' choices at the morphosyntactic stratum, realized, in turn, by 'sounding' choices at the phonological stratum. It is recognized that in some instances, particularly as regards certain features of intonation in English, phonology can be seen directly to realize semological choices, and that relevant features of the situation of discourse can be communicatively manifested non-linguistically.

Relationships between the planes of experience and the strata of the code or 'grammar' are indicated in the following figure:



NOTE: Language behaviour actually happens; planes of experience and strata of the 'grammar' are fictions of the linguist.

Figure 1

In this paper sentence and clause will be used as terms for units in the morphosyntactic rank scale for English, related to the semological units, proposition and predication. It will be suggested that sentence, as described here, is also a crucially important unit in the analysis of discourse.

My starting point is, I think, compatible with systemic orthodoxy. The independent clause in English is the realization locus for simultaneous systems and structures which reflect language's experiential, interpersonal and textual or, as I prefer and will henceforward use, discourse functions. These semological systems and structures configure to yield a morphosyntactic unit which is analysable in terms of Subject, Predicate, Complement and Adjunct elements. Predicational systems and structures in the semology concerned with different kinds of processes and relations and their attendant participant rôles and circumstances are relatable to systems and structures of transitivity, voice and adjunctivization (involving the presence and absence of different kinds of Predicate realizations, Complements and Adjuncts) in the morphosyntax.

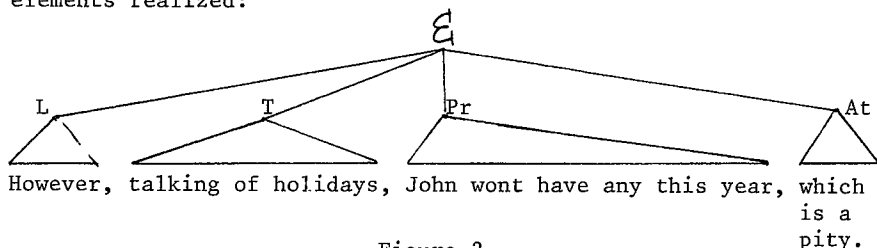
Systems and structures in the semology concerned with speech act distinctions, such as those amongst statement, question and command, or between comment and directive, are relatable to morphosyntactic choices of mood involving such features as the position or absence of Subject, the use of 'let', and of tags in the clause yielding distinctions, with the possibility of further delicacy, between indicative and imperative; declarative and interrogative; polar and WH interrogatives; optative and jussive imperatives.

Semological systems and structures concerned with message organization and matters of focus and prominence are related to morphosyntactic clause systems of theme, marked and unmarked, and to information systems concerned with what is presented as 'new' and what is presented as 'given', in the phonological tone group (cf. Halliday 1967, 1968; Grimes 1975). Halliday (1980) sees theme as 'user' prominence and new as receiver prominence.

Independent clause in English is then a morphosyntactic unit which simultaneously realizes a semological predication, speech act, and organized message. The combination of predication, speech act and organized message is what we recognize as a proposition in the semological rank scale which also includes concept and component, and in a limited sense, predication (cf. Beekman, Callow and Kopesec, op. cit. for a similar position).

Nuclear to the structure of the sentence as morphosyntactic unit is the element which the independent clause typically realizes. This we call the Propositional element. As regards other elements in sentence structure the position is similar to Pike and Pike's. They write: 'we would like for clause... to include those tagmemes which relate certain participants to each other and to an action or state within elements of its marginal setting such as benefactee or time, place, manner. Tagmemes will be considered outside the clause but within the simple sentence if they add other kinds of relationship or if

it seems clear to us...that the further tagmemes are modifying the clause as a whole, rather than some one tagmeme of that clause (Pike & Pike, op. cit.:257). In our proposed analysis, as well as the Propositional element (Pr), three other elements are recognized at primary delicacy: Link (L), Attitudinal (At) and Topic (T). All can be justified as elements of structure by customary morphosyntactic criteria of componence and distribution, slot and filler. And all have rich discourse significance. Figure 2 shows an example from conversation with these elements realized:



Link, which usually occurs towards the beginning of the sentence, although final position is possible in spontaneous speech, has a very clear discourse rôle: items such as therefore, however, nevertheless, meanwhile, hereafter, hitherto, meantime, later, earlier, and and and but when they are not operating in syntactic parataxis signal what in Pike's terms is a dependent sentence. Because in my terms sentence is the top unit of the morphosyntactic rank-scale, I prefer to talk of a discourse dependent sentence: one that is not usually a discourse initiating sentence or the sole realization of a discourse. While analyzing children's conversation, Malcom and I discerned a possible secondary structure at the Link element which is place-ordered. The examples of Link I have given, which might be called Conjunctive Links, can be preceded by Polarity Links: Dissent Links such as no, nope, and Assent Links such as alright, yeah, O.K. and compounds such as yeah O.K., well O.K. So we had sentences such as Yeah O.K./but/I don't want to do that (Assent.Link/Conjunctive Link/Proposition). There are, of course, other ways of handling such phenomena (clausal ellipsis, for example) and it could be that, although in the code polarity labels may be appropriate, in instances of discourse such items operate primarily as silence or space fillers.

Topic element realizations, such as in reference to your letter, in case of a delay, as for Bill, as regards our journey, as I was saying, to return to the question, need a different treatment from marked theme within an independent clause. They are clearly important, in their own way, in discourse; frequently they both refer back in contradistinction and forward in anticipation. There are clear morphosyntactic differences between sentence topic and clause theme. Topic is not open to being

present and unmarked: it is either there or it is not. Clause theme, on the other hand, may have attention drawn to it, i.e. he marked, or it may not. 'I saw Bill yesterday.' (unmarked); 'Bill, I saw (him) yesterday.' (marked). Furthermore 'Bill I saw yesterday.' and 'Bill, I saw him yesterday.' contrast with 'As for Bill, I saw him yesterday' non-trivially. In the first two there is no difficulty in regarding Bill as the realization of an element of clause structure, a theme-marked objective complement. In the third example, the as for initiated group structure makes such an analysis morphosyntactically difficult. Nor would any of the examples of Topic I have met, or any cited in Pike and Pike, sit easily with an analysis of marked adjunct theme, that is, as the morphosyntactic realization of some kind of circumstantial rôle under focus in the semological predication. Rather they serve to switch, initiate or reinforce the experiential orientation of the communication which relates them to the discourse implications of a sentence and not to the internal organization of a clause. So I do not agree with Pike and Pike (op. cit.:257) when they regard Bill in 'Bill, I met yesterday.' as in a double function across levels (ranks in systemic terms): as their fronted undergoer in clause root and as their sentence margin topic. The systems of clause theme and sentence topicalization can make a necessary, if fine, distinction between 'Bill I met yesterday' and 'As for Bill, I met him yesterday.'

Link and topic elements relate to the discourse function of language. The Attitudinal element is clearly a reflection of the interpersonal function and can be seen to place the process or relation, participants and circumstances the Propositional element realizes as a predication in a framework of communicator assessment. This means that Attitudinal elements have implications for the continuing thread of discourse's inter-actional aspect, implications which need fuller exploration.

At secondary delicacy in the morphosyntax a chain structure involving final and mobile Attitudinal elements can be distinguished. Final Attitudinals (Atf), typically realized by which initiated clauses or by nominal groups with evaluative lexis, follow the Propositional Element:

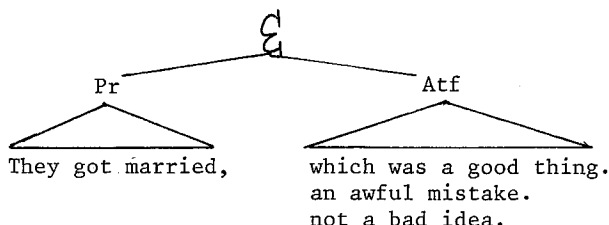
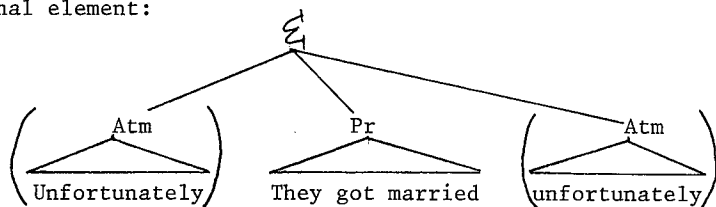


Figure 3

Mobile Attitudinals (Atm) realized by evaluative adverbials usually occur before, but may also occur within; or after, the Propositional element:



or

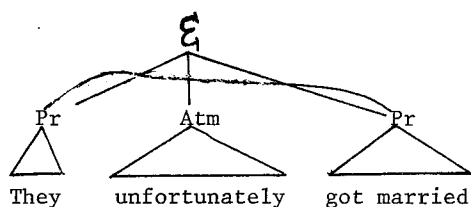


Figure 4

Mobile Attitudinals realized by modalizing parenthetical clauses may occur in the included or final position: 'He is, I suppose, the best person for the job.', 'He is the best person for the job, I suppose'.

It is proposed then, that sentence is the most appropriate unit in the morphosyntax at which to discern the ranges of choice users of English have to realize their 'attitude' to a Propositional element and to 'link' it to, and 'topicalize' it in relation to, both previous and anticipated discourse.

The Propositional element itself may be realized by a single independent clause, as in the examples cited so far, or by a compound as in:

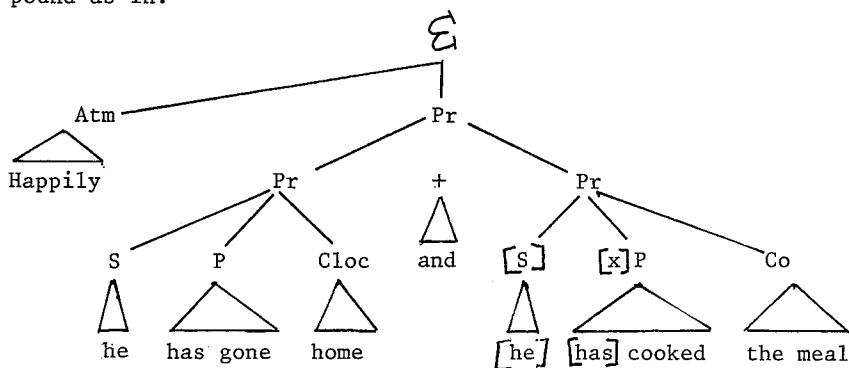


Figure 5

Again following Pike and Pike, I refuse to treat a formally independent clause such as 'John is coming' as in fact structurally independent if it is semologically tied for its truth value to, or tempered by, another clause. So a propositional element can be realized by a clause complex with a $\beta\alpha$ relationship between the clauses. Predicationally in the semology, the proposition is a complex but as regards speech act it is a simplex as only the clause at α enters into mood choices:

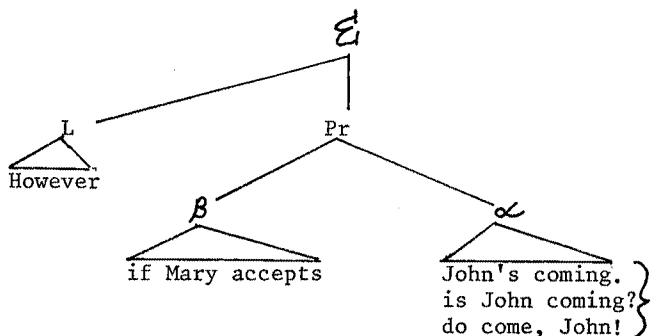
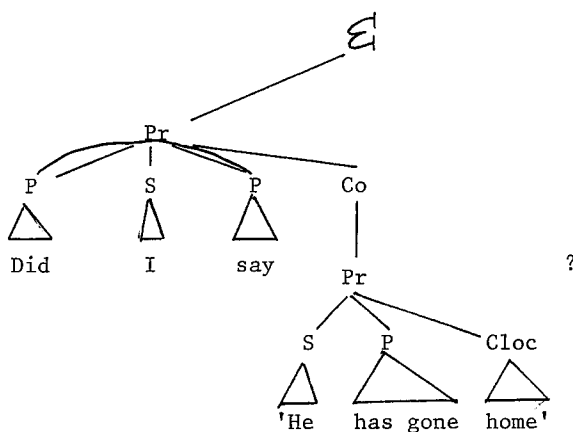


Figure 6

Not only condition-contingency but also cause-effect, point-of reference - contrast, and purpose - proposal complexes are treated the same way (cf. Pike & Pike, op. cit.: 259 for a similar if not identical view). The α clause is said to be 'tempered' by the β clause. If, as has been proposed, independent clauses are, by way of their transitivity, mood and theme selections, the morphosyntactic realization of semological propositions which are simultaneously predications, speech acts and fully organized messages, then 'tempered' clauses (at α) are quasi-propositional semologically because they are predicationally incomplete. 'Tempering' clauses (at β), like dependent clauses, although predicationally do not themselves realize a speech act through a mood selection and are restricted as regards the range of choices they can select from in discourse organization and focus.

In the case of a quotation placed in the framework of a report both the 'quote' and the 'quoting' parts are fully propositional so the quote is represented as an embedded (rank-shifted) sentence:



and

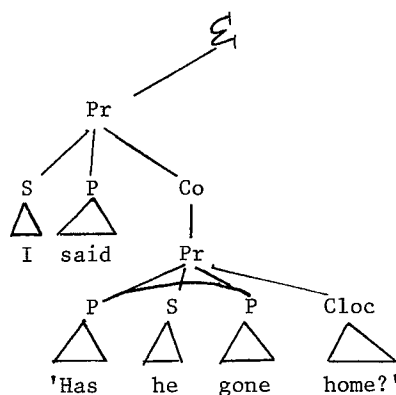


Figure 7

Michael Halliday in two insightful and detailed articles, 'Modes of Meaning and Modes of Expression' (1979) and 'Text Semantics and Clause Grammar' (1980) has delineated the semantic and discourse implications and importance of the clause unit. Sentence, however, can be distinguished from clause on morphosyntactic criteria of the slot and filler kind. As a morphosyntactic unit it has a different semological significance from independent clause. Independent clause is the morphosyntactic realization of proposition; sentence places a proposition or propositions in the contexts of discourse and of speaker's assessment. It is therefore, I propose, not only the most inclusive unit in the morphosyntactic rank scale for English within the stratal organization of language as complex code, but also a key unit in the analysis of the discourse plane of experience.

In tackling the problem of describing language as intentionally communicative behaviour, I suggest that it is not clause as the realization of proposition that is the bridge unit between codal potential and discourse actualization but sentence which can link, topicalize, and attitudinalize propositions. It is the 'package' which offers clues for the recognition of phase and transition in English discourse (cf. Gregory and Malcolm, forthcoming) and has important implications in the assessment of paragraph as a discourse unit (cf. Longacre 1976: 262 & 279, Malcolm 1982)

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INDIRECT OBJECT STRUCTURES AND LANGUAGE ACQUISITION

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1. Introductory Remarks. As a first approximation, it seems reasonable to suggest that the search for an explanatory account of the language acquisition process will make reference to three things:

- (1) the grammar attained and used by mature native speakers of the language in question;
- (2) the experiential data that learners draw upon in the course of acquiring their native language;
- (3) the structure of the language faculty—i.e., the mechanisms, principles and strategies used by learners to analyze and exploit experiential data in the course of language acquisition.

For the past two decades, transformational grammarians have been drawing on assumptions about the nature of the grammar and of the experiential data to make inferences about the character of the language faculty. Typically, the line of argumentation proceeds as follows. A principle of TG (say, the Subjacency Condition) is selected and is assumed to be an actual part of the speaker's knowledge of his language. The experiential data is then examined and is shown to be too impoverished to allow acquisition of the principle in question by any known process of learning. It is therefore concluded that the principle must be innate—i.e., an intrinsic part of the language faculty. (Chomsky (1980:34) calls this 'the argument from poverty of stimulus')

I do not believe that there is any inherent methodological flaw in this approach to language acquisition. There is little doubt that if we did have a basic understanding of any two of the three phenomena associated with language acquisition, we could gain important insights into the character of the third. However, I would like to make a somewhat different use of this methodology, making certain assumptions about the character of the experiential data and the language faculty in an attempt to gain insights into the nature of the grammar. The two most critical assumptions that I will make about the properties of the experiential data and the language faculty are stated in (4) and (5).

- (4) The experiential data relevant to the language acquisition process consists primarily of utterances used in situations where their meaning can be inferred from prior linguistic knowledge, pragmatic considerations, contextual information and so on.
- (5) The language faculty does not include any innate linguistic knowledge, but rather consists entirely of principles and strategies that could be expected to manifest themselves in other cognitive domains as well (e.g. the Transitivity Principle of logic, deductive inference, inductive generalization, etc.).

Assumption (4) is relatively uncontroversial and is generally supported in the literature on language acquisition (e.g., Macnamara 1972, Cross 1977). Assumption (5), on the other hand, is a somewhat extreme position and is much less likely to receive wide support. This notwithstanding, I believe that it can be fruitful to initially adopt a strong—or even extreme—position like (5) and to follow it through to its logical conclusion. Even if it ultimately proves untenable, the precise nature of its shortcomings can provide valuable insights into the phenomenon to which it is applied.¹

This said, let us now turn our attention to one of the most fundamental problems in linguistic theory, namely the nature of grammatical relations (GRs).

2. **The Problem of Grammatical Relations.** The underlined NPs in sentences (6)–(8) differ in at least one important respect from those found in (9)–(11).

- (6) Mary left.
- (7) Mary read a book.
- (8) Mary sent John a book.
- (9) Mary stood near Harry.
- (10) Mary gave a book about Harry to Max.
- (11) The book was read by Max over a period of three hours.

Notice that whereas the relation of the underlined NPs to the rest of the sentence is made explicit by a preposition with some sort of independently identifiable meaning in (9)–(11), there is no overt indication of the role played by the nominals found in (6)–(8).² The nominals in this latter class are typically said to enter into a GR with the verb; in the sentences in question, Mary is taken to be subject (S), book to be direct object (DO) and John to be indirect object (IO). I consider a nominal like Max in (10) to be part of a locative PP rather than an IO.

Two remarks can be made about the child's acquisition of GRs. First, the S and DO relations appear to be mastered relatively early in the language acquisition process; SV and VO structures

like Mommy go and [I] see Daddy are used with the appropriate intended meanings from early in the two-word stage (beginning around 18-24 months). Second, the IO structure exemplified by (8) does not seem to be fully acquired until several years later, around age 7 or 8 (Roeper et al. 1981). This is important since it suggests that children do not equate GRs with semantic roles. If they did, it is hard to see why they would have difficulty learning that IOs typically denote recipients just as Ss do agents and DOs do patients.

Our problem, then, is to develop an account of GRs which is consistent with assumptions (4) and (5) and which accounts for the developmental facts just noted. One suspects that if we are successful in this endeavor, we will also have gained insights into the character of grammar which ultimately underlies linguistic activity in adults.

3. A Characterization of Grammatical Relations. It has been consistently assumed within TG (e.g., Chomsky 1965, 1981) that GRs are defined in terms of the abstract properties of conventional tree structures.³ Under our assumptions, however, this analysis must be rejected since it is widely believed (e.g. Jackendoff 1977) that important properties of the tree structures necessary to Chomsky's definitions must be innately specified. Consistent with proposals I have made elsewhere (e.g. 1980, to appear), I would like to suggest that the child formulates syntactic definitions and principles in terms of 'predicational structure'—a level of representation depicting the combinatorial relationships among the meanings of the words and phrases that comprise an utterance.

As I envisage it, predicational structure provides a representation of the type of relationship found in a phrase like red house and paraphrased in (12).⁴

(12) The property of being red is attributed to some house.

We can use the diagram in (13) to represent this predicational relationship.

(13) 
red house

Let us say that in (13), red 'predicates' house.⁵

It is my claim that in the course of language use children compute predicational relationships of the sort depicted in (13) rather than conventional syntactic structures. Notice in this regard that the discovery of predicational relations by the language learner does not presuppose innate knowledge; it should suffice for the child to have noted that language includes words with identifiable, more or less fixed meanings which can be com-

bined with each other to express complex messages. Thus, a child who understood the meanings of red and house and who observed the phrase red house being used to refer to a residence of that color should be able to infer that the adjective has been used to predicate the noun (i.e., that the property of being red is attributed to the house).

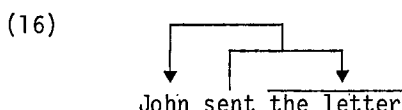
Since predication structure is (by hypothesis) the only level of 'syntactic' representation available to language learners, we can assume that they will seek to understand GRs in terms of it. Interestingly enough, there is good reason to believe that subjects and objects are distinct from other nominals in being predicated by the verb or a unit of which the verb is a part. Consider in this regard a sentence like (14).

(14) John sent the letter.

As a first approximation, it would be correct to state that the interpretation associated with (14) stems from its expression of the predication relations paraphrased in (15).

- (15) a. It is said of the letter that it was sent.
b. It is said of John that he sent the letter.

These predication relations are represented in the diagram of (16).



The diagram in (16) depicts John as 'topic' (what the rest of the sentence is about). There is no logical reason why this has to be so, but I will assume that it is in a large number of cases and that the discourse context is rich enough to allow the language learner to identify the subject as topic in the appropriate cases. There is, however, another feature of subjecthood that goes beyond properties of predication structure: the S of an active sentence also denotes the 'actualizer'—the entity which is most actively involved in the development of the event denoted by the verb. This fact manifests itself in the different paraphrases of the subject and object relations in (15).

The preceding attempt to analyze GRs in terms of predication relationships allows us to offer the following tentative characterization of the emergence of the S and DO relations. Based on their experience with language, children will notice that words denoting actions, states and processes (i.e., verbs) consistently occur next to words denoting individuals (i.e., nouns). Children will become aware of the predication relationships underlying GRs at the point at which they realize the role which

the individuals denoted by the nominals play in the action encoded by the verb. That is, once a child realizes that in a sentence like (14) letter denotes the object that was sent, he will also realize that it is being said of the letter that it was sent (i.e., that send predicates letter). Since it is well known that young children understand the structure of actions and since there is reason to believe that adults often use sentences in contexts where the action being encoded can be easily identified, mastery of the predication relations associated with the S and DO should occur early in the language acquisition process. As noted earlier, this is what happens.

Consider now the IO relation manifested in (17).

(17) John sent Mary a book (Mary = IO).

As noted above, structures like these do not appear to be mastered until 7 or 8 years of age. A puzzling aspect of this phenomenon is that the semantic role of recipient does not seem to create difficulties for the child since the use of locative PPs to express this relation (as in (18)) is common from relatively early in the language acquisition process (from around age 3 - Waryas & Stremel 1974).

(18) John sent a book to Mary.

Although it has been suggested from time to time that the difficulty of sentences like (17) relates to the occurrence of two NPs in a row, this explanation is less than convincing. Certainly, 5- and 6-year old children encounter no problems with sentences like (19) (which contain two consecutive NPs, each with an /s/ ending):

(19) The boy's brothers are outside.

Moreover, it is well known that from a young age (around 30 months) children can make use of word order to uncover semantic relations in other constructions.

It is natural to consider the possibility that properties of predication structure might contribute to the late acquisition of the IO structure exemplified by (17). Of particular interest in this respect is the fact that the interpretation of the predication relationship associated with the IO is unique in that it must invariably make reference to another predication relationship, namely that between the verb and the DO. Thus, notice that the predication relation between the verb and the IO in (17) must be paraphrased as (20a) rather than (20b).

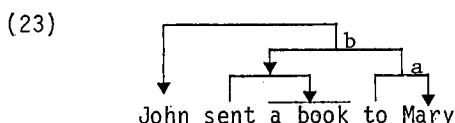
- (20) a. It is said of Mary that she was sent the book.
 b. *It is said of Mary that she was sent.

Interestingly enough, there is no comparable implicational constraint on either the subject relation or the direct object relation. Thus, we have sentences like (21)-(22) in which the interpretation of the predication relation associated with an S and a DO (respectively) will not make reference to another GR.

(21) John left.

(22) It is easy to read that book.

Now consider the predication relation associated with the locative phrase in (18). Assuming the relevance of a sentence's surface form, it seems reasonable to suggest that the verb send does not predicate Mary in (18) as it does in (17). Rather, in (18), Mary is predicated by to and the resultant phrase—a locative (adverbial) PP—then predicates the VP.



The relevant portions of (23) (marked (a) and (b)) can be paraphrased as follows.

- (24) a. Mary is specified to be goal of some action.
 b. The 'goal' of the action of sending the book is specified to be Mary.

Notice that the locative to-phrase resembles the S and the DO in that the interpretation of its predication relationship need not make reference to the realization of any other predication relationship. Nothing changes if we delete mention of the direct object book in (24b) (unlike (20)) and there are many sentences like (25)-(27) in which there is a to-phrase but no direct object.

(25) We talked to John.

(26) John went to New York.

(27) They gave to charity.

The predication relationship associated with the locative phrase in (25), for example, can be paraphrased as in (28). Notice that no reference is made to any other predication relationship.

(28) The 'goal' of the talking is specified to be John.

We are left, then, with an interesting fact: IOs are unique in being associated with predication relations whose interpretation requires reference to another predication relationship. It seems plausible to suggest that this characteristic of IOs contributes to their late acquisition.

4. **A Semantic Contrast.** One more point remains to be made. If I am correct in claiming that the verb predicates Mary in cases like (17), but not (18), then one would expect this contrast to manifest itself in some sort of semantic distinction. I believe that this does happen and that there is a discernable tendency for a goal or recipient encoded as IO to be seen as more directly affected by the verb than one encoded as a locative phrase. Let us consider seven representative examples of this putative contrast.

- (29) a. I'll teach those students linguistics come hell or highwater.
- b. I'll teach linguistics to those students come hell or highwater.
- (30) a. Gretta showed Sam the meaning of true love.
- b. Gretta showed the meaning of love to Sam.
- (31) a. Can you spare a poor hippy a dime?
- b. Can you spare a dime for a poor hippy?
- (32) a. She handed John her hat.
- b. She handed her hat to John.
- (33) a. She gave John a cold.
- b. ??She gave a cold to John.
- (34) a. John sent Mary his love.
- b. ??John sent his love to Mary.
- (35) a. John gave Harry a broken arm.
- b. John gave a broken arm to Harry.

Examples (29)-(31) are from Green (1974) and example (32) from Miller (1978). My remarks on the contrasts found in these sentences are based on their comments.

The sentence pairs of (29)-(35) are most easily interpreted in the predicted way, with the referent of the IO being seen as more tangibly affected by the action denoted by the verb than is the referent of the locative phrase. In (29a), for example, the speaker seems to be determined to have the students actually learn linguistics rather than just hear lectures on the subject (which is the more obvious interpretation of (29b)). Similarly, in (30), the (a) version suggests that because of Gretta, Sam actually experienced a true understanding of love; in contrast, the (b) version implies that Gretta simply exposed Sam to a technical description of that emotion. An even subtler contrast seems to underlie sentence (31) since it is easier to interpret the poor hippy as the person who utters the (a) version than the (b) version. With regard to the sentences of (32), some speakers feel that whereas the locative structure 'merely suggests that John

took the hat,' the IO construction 'asserts' that he did (Miller *ibid.*:86). The sentences of (33)-(34) represent a slightly different phenomenon in that the locative structure is actually less grammatical than its IO counterpart rather than just being different in meaning. These facts would follow from the assumption that expressions like send one's love and give a cold take recipients which are very much like patients (very tangibly affected by the action denoted by the verb). Finally, notice that whereas (35a) implies that Harry had his arm broken, its (b) counterpart suggests that he was handed a broken arm (perhaps from a doll or a mannequin).

Although there is doubtlessly considerable flexibility and variation in judgments pertaining to sentences (29-35), it seems to me that there is an undeniable tendency to interpret the referent of an IO to be more patient-like than the referent of a locative phrase. This contrast has a double relevance for my analysis of GRs. Not only does the predication characterization predict the tendency exemplified in (29-35), it also contributes to an account of its acquisition. Notice that the contrast under discussion is extremely subtle and rarely manifests itself in a categorical way. It is therefore hard to believe that the experiential data alone would be sufficiently clear and rich for children to simply induce the existence of this contrast in the same way that they might the phenomenon of subject-verb agreement (for example). It is, of course, even more implausible to believe that the contrast could be innate.

The learnability problems associated with the IO-locative contrast would be considerably diminished if it could be shown that this phenomenon did not really have to be learned at all but rather followed as a 'natural consequence' of more basic grammatical facts. I believe that this is the case and that the IO-locative contrast can be shown to virtually follow from knowledge of the meaning of to and from an understanding of the basic properties of predication structure. Of particular importance in this respect is the fact that the preposition to is frequently used to indicate a relatively indirect relationship between an action and a goal. Notice that in sentences like (36)-(37), for example, there is no reason to believe that action is ever carried through to its goal.

(36) John started to the store.

(37) I spoke to John (but he didn't hear me).

One might suppose that an understanding of to's expressive value in these constructions might provide the child with a basis for assuming that it also expresses a relatively indirect relationship between the action and the goal in structures like (29b)-(35b). In the case of the IO structure, on the other hand, the predication relation between the verb and the IO is not 'mediated' by an intervening preposition and it would be natural for the child

to assume that the action being predicated of the goal or recipient affects it in some relatively tangible way. This is precisely the contrast that seems to exist.

5. **Concluding Remarks.** In my preceding remarks, I have attempted to deal with what I believe to be a number of interrelated issues—the early appearance of Ss and DOs in child language, the early use of locatives to denote recipients, the late acquisition of IO structures, and the possible existence of a semantic contrast between IO and locative constructions. Working under the assumption that the language faculty does not include innate linguistic principles, I have tried to show that it would be natural for the child to characterize GRs in terms of predication structure. I noted that this hypothesis not only accounts for the late appearance of IO structures but also for the contrast which seems to distinguish them, in some cases at least, from comparable locative constructions.

I realize, of course, that none of my proposals are unproblematic and that I have only been able to touch lightly upon a number of important issues and phenomena. This notwithstanding, I believe that one fundamental point is beyond challenge: rejection of conventional beliefs about the necessity of syntactic structure and the existence of innate linguistic principles can open up interesting—and even exciting—new possibilities for our understanding of both language acquisition and its product, linguistic competence.

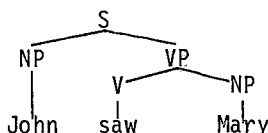
NOTES

¹It is important to note parenthetically that the assumption in (5) does not constitute an endorsement of empiricism in general or behaviourism in particular. There is no reason to believe that grammatical knowledge results from the simple observation and recording of associations, the unmediated internalization of experience, and so on. I follow Piaget (1972) in believing that the ontogenesis of knowledge is characterized by the active construction of successively more sophisticated cognitive structures. For more on this, see O'Grady (to appear).

²Notice that word order allows us to keep the functions of those nominals distinct, but it does not indicate what the different functions are in the first place.

³In a structure like (i), for example, the subject is taken to be the NP immediately dominated by S and the DO the NP immediately dominated by VP.

(i)



⁴Here and elsewhere the paraphrases of predication relationships are meant for expository purposes only and their form has no theoretical significance.

⁵We might make precise the notion of predication by defining it as follows:

An element X predicates an element Y if X, or some part of X, denotes a function which takes Y as an argument.

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A THEORETICAL BASIS FOR GERMAN WORD ORDER

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0. In the year 1973 Helmut Esau published a minor article in Linguistics on "The Order of the Elements in the German Verb Constellation" in which he posited an underlying verb last configuration in the German language with a fronting transformation to account for the verb-second order of the main clause. He recognized the weakness in his own treatment of German word order, however, pointing out that his hypothesis did not explain why the verb in the subordinate clause was not subject to the fronting rule, but he suggested the possible existence of another force that prevented the fronting transformation from being generalized. Helmut looked upon his formulations as "guidelines for further research rather than as answers to the questions raised"(p. 39). Helmut moved on to other subjects and did no further research on the problem of German word order before he was killed in an automobile accident in December of 1981, but he left me a legacy in this article, because although we collaborated on several papers in other areas, it was this essay that introduced me for the first time to the fascinating subject of word order universals in general and German word order in particular. In this essay I have tried to answer Helmut's question about the 'other force' and so it seems quite appropriate to dedicate it to the memory of a devoted linguist and friend.

1. Introduction

Greenberg(1966) has shown fairly conclusively that universals of language do exist and that the order of elements in a sentence are in some way tied to the position of the subject and object relative to the verb, although many typologists now work with a later refinement of the typology into OV and VO languages. The existence of such universals raises some important questions, some of which I have discussed in an earlier paper(1979). If the two main language types do exist, then it would seem to imply that over a period of time a language tends to develop toward a given type. But if a language does tend to develop in the direction of one word order type or another, then it implies that some characteristic of the language grammar must in effect be determining the future development of the language, otherwise there is no logical basis for the language to continue to develop in a given direction other than by coincidence. To admit chance as a basis for universal patterns in language is to forfeit any legitimate claim to the recognition of linguistics

as a scientific discipline. Consider, for example, a language which at point A in its history has 90% of the OV features and 10% of the VO features. When new features develop within the language, there is an even chance for those structures to be consistent with either OV or VO languages. Thus without external influence there is no logical reason for any language to develop into a consistent word order type unless that language is conducive to structures of that type to begin with. That in turn implies that the grammar itself must be structured in such a way that it either generalizes structures of that type or is conducive to accepting those structures when they become available to the language. This conclusion receives support from Hawkins' reevaluation of Greenberg's data and his Universal Consistency Hypothesis, according to which:

At each stage in their historical evolution, languages remain consistent with synchronic universal implications.

It should be noted here that Hawkins' universals are based on a refinement of Greenberg's SVO/VSO/SOV hierarchies and that he explicitly rejects the OV/VO division. In fact, it seems quite probable that if linguists can ever come to an understanding of what causes the development of universal patterns in word order, then we will have made a major step toward a legitimate theory of language. For that reason I consider word order typology to be one of the major linguistic developments in this century and have established it as a part of the foundations of my own linguistic research.

Any real theoretical developments in universal word order theory must of course be based on a comparative analysis of and application to many languages, but as a beginning there is no better language to apply the theory to than German. The contradictions between the OV order of the subordinate clause and the VO order of the main clause have placed the language right in middle of word order research and have made it a very controversial language, because typologists cannot decide whether to consider it a VO language, an OV language, or a messy mixture of both. Consider, for example, Vennemann's description of German as a language with a "very bad syntactic system due to its incomplete shift from SOV to SVO" (1973, 46). It is difficult to imagine that a syntactic system that is as clearly definable as the German system is, is really a "very bad syntactic system", and in section 4 I will show the rules that have determined the development of the system and that the system is not quite as mixed up Vennemann thinks.

2. Focus Grammar

Focus Grammar has its origins in various different areas of both applied and theoretical linguistics and at the present time represents more a methodology than it does a theory. An important source of influence of focus grammar is the study of

language universals and linguistic typology. In fact, one might even say that the very heart of the theory is directly derived from a desire to explain the origins of language universals.

A second area of influence in the development of focus grammar has been the nature of scientific theory and the role of linguistics as a scientific discipline. I tried to establish one of the main features of my methodology in Barnes(1980). In that paper I showed that if linguistics is to function as a science, then the linguistic rules should be the linguistic equivalent of the formula and equation in modern physics and chemistry. As such these rules should represent the mental or cognitive processes through which the language itself was generated. Specifically, focus grammar attempts to make general assumptions about the nature of language based on the speech act, language learning, and language universals and to determine what kinds of linguistic rules are necessary in order to account for these phenomena. If this effort is successful, then the results can be used to derive descriptive rules for the individual languages within the context of the model.

In developing the focus grammar model, I have tried to make the simplest possible basic assumptions. I assume that the major function of human language is to communicate the speaker's perception of reality to the listener, whether or not that listener is someone else, as in the case of conversation, or the speaker himself. Thus the goal of language is to recreate in abstract form the reality of the world as it is perceived by the speaker. Logical syntax provides an arbitrary means to represent the real world; the linguistic rules then convert this semantic structure into the syntactic form and phonological body of the human utterance. Note that the propositional form of the logical syntax is already an abstraction within the model for the states and events of reality. This step is consistent with and required by the methodological assumptions made in Barnes(1980), according to which every element in the linguistic grammar must correspond to some aspect of the communication process. This semantic structure does not include such pragmatic elements as discourse structures or topicalization, all of which I include under the rules of language rather than in the representation of reality.

But what kinds of rules are necessary in order to generate a sentence from a semantic structure?

Consider the semantic structure in (1): there are two different kinds of rules needed in order to maintain the meaning of the elements of the proposition as well as the relationship between those elements.

(1)a. The man gave the dog to his son.

b. GIVE(MAN,DOG,SON)

If we assume the simplest possible one-to-one relationship between the individual morphemes and their meaning, then there is one

important requirement that must be met in order to maintain the meaning of the proposition in the surface representation; one must have some means of indicating the relationship between the various arguments and the verb itself. This being the case, I have established as a basic principle of focus grammar the requirement that there must be underlying rules of the form

- (2) $X \Rightarrow \text{Head} + \text{FM}$
 or
 $X \Rightarrow \text{FM} + \text{Head}$

in which the function marker(FM) of each sentence element is ordered relative to the element itself. This rule may be perceived as a perceptual strategy rather than a strictly syntactic rule, and it may result in prepositions, postpositions, or inflection, but it will in every case tend to result in a single ordering relationship between the sentence element itself and its role marker.

Note particularly at this point that rule (2) excludes the common perception of the prepositions as predicates. Although there might be exceptions, rule (2) suggests that prepositions are strictly syntactic and not part of the semantic structure and thus of the logical syntax.

The second major principle necessary for communication involves the relationship between a given element and its modifiers. Theoretically one could mark this relationship with a function marker as well, but in practice the relationship is normally marked through word order, as in (3)

- (3) $X \Rightarrow \text{Head} + \text{Modifier}$
 or
 $X \Rightarrow \text{Modifier} + \text{Head}$

Once again the rules in (3) must be perceived as perceptual strategies that tend to operate in the formulation of specific rules, both in speaking and in understanding.

It should be noted that the rules (2) and (3) have separated under two rules what is normally classified in universal typology under a single rubric. This separation of modifiers and function markers is furthermore derived from the underlying theory of language and not from the distribution of language data.

3. Topicalization

In addition to the above mentioned basic processes of syntax, pragmatic processes such as focus and topicalization also play an important role in focus grammar. One such process, topicalization, plays an important role in the German word order problem that I will discuss below.

Probably all languages have some means of marking the topic of a sentence, but not all languages have overt morphemes for

that purpose, as does for example Japanese. Consider the sentences in (4). In the first sentence the particle ga marks the subject of the verb and the particle o marks the object. But either of these particles can be replaced by the topic marker wa, as is illustrated in the other two sentences in (4).

- (4)a. John ga kore hon o yonda
 John SUBJ this book OBJ read
 'John has read this book.'
- b. John wa kore hon o yonda.
 'Speaking of John, he has read this book.'
- c. Kore hon wa John ga yonda.
 'Speaking of this book, John has read it.'

In VSO languages the role of topic marker is apparently taken over by the inflected verb. Both Arabic and Welsh, for example, allow topicalized elements to precede the verb. Consider the Welsh examples in (5).

- (5) a. Ar y gornel y mae siop fach.
 on the corner PART is shop little.
 'On the corner is a little shop.'
- b. Y mae'r siop fach ar y gornel.
 PART is-the shop little on the corner
 'The little shop is on the corner.'

In (5a) the adverb is topicalized and fronted; (5b) represents the unmarked word order.

There is one additional aspect of languages that must be considered before proceeding to the German word order system. Sentences can take particles or function markers just as nouns do. In OV languages the sentence particles tend to follow the verb at the end of the sentence just as the noun particles tend to follow the noun. Consider the Japanese example in (6) where the particle ka indicates that the sentence is a question.

- (6) Kore wa hon desu ka? 'Is this a book?'

In VSO languages, on the other hand, the particle tends to precede the verb in the first position, as is illustrated by the Welsh examples in (7).

- (7)a. Yr wyf i yn ysgrifennu llyther.
 PART am I in writing letter
 'I am writing a letter.'
- b. A ydych chi yn darllen llyfr yn awr?
 ? are you in reading book now
 'Are you reading a book now?'

In these past two sections I have outlined some of the basic features of focus grammar and some of the typological characteristics of SOV and VSO languages that are relevant to the problem of German word order. Among others, I have pointed out the need to generate the utterance from a semantic structure, the need for grammatical rules to parallel the psychological processes that generate the sentences, and the need for two basic types of rules in order to maintain the meaning of the semantic structure in the resulting utterance. Particularly I have shown the role of the sentence particle in OV and in VSO languages and how that particle and the fronted verb serve double duty as a topic marker. All of these characteristics will also play a role in the following interpretation of the German word order data.

4. German Word Order and the Bracket Construction

The facts on German word order are very simple to describe, but somewhat more difficult to explain. They are well-known in the literature on typology, primarily through the writings of Lehmann and Vennemann. German is of special interest to typologists because it shows characteristics of both OV and VO languages, so that there has been much discussion as to what the basic word order is.

The major problem in German is the fact that whereas the independent clause has a verb-second order, the subordinate clause has a verb-last order. This problem is complicated further by the fact that German has been adding an ever increasing number of characteristics of an OV language despite the fact that the inflected verb is in the second position in the main clause. There appears to be one exception to the OV developments, however, and that is the preposition. Whereas in the past the language has accrued numerous postpositions, at the present time these postpositions are being converted to prepositions: des Wetters wegen => wegen dem Wetter; dem Autor nach => nach dem Autor, etc. I refer the reader to the literature for further information on the subject.

In German the inflected verb is always in the second position in the sentence, except in yes/no questions and imperatives. In the subordinate clause, on the other hand, the inflected verb always occurs at the end of the sentence. Infinitives and participles are always at the end of the clause with only the inflected verb following. This rigid placement of the verb has resulted in a 'bracket' structure as illustrated in (8).

- (8) Der Lehrer hat gar nichts über Wortstellung gesagt.
'The teacher said nothing at all about word order.'

Ich will noch mit dem Lehrer sprechen.
'I still want to speak with the teacher.'

Er sagte, daß er mit dem Lehrer sprechen will.
 'He said that he wants to speak with the teacher.'

Thus the question arises as to which of the above word order patterns is the basic pattern in German—the VO structure of the main clause or the OV structure of the subordinate clause. The literature on the topic is now becoming quite extensive.

There is another aspect of the bracketing structure that has been thoroughly discussed in the descriptive literature on German but which has received less attention from typologists, and that is the extent to which the bracket pattern has permeated the German sentence structure. Consider for example the subordinate clause, which must always begin with either a conjunction or a relative pronoun, as in (9). German descriptive linguists commonly recognize this pattern as part of the bracket structure.

- (9) weil ich noch mit dem Lehrer sprechen will.
 because I still with the teacher speak want to
 'because I still want to speak with the teacher.'
- (der Mann), den ich gestern gesehen habe.
 (the man), whom I yesterday seen have
 '(the man), that I saw yesterday'

Consider also the prepositional phrases, particularly ones indicating direction, which often seem to be growing a tail through the addition of an adverbial particle:

- (10) auf den Berg hinauf 'up the mountain'
an der Straße entlang 'along the street'

Even the noun phrase has a kind of bracketing structure, with the article serving as the first bracket and the noun as the second bracket.

- (11) der große Mann
 the big man

der große, rothaarige, gar nicht zu liebende Mann
 the big redhairednot-at-all-to-be-loved man

The brackets are thus a very pervasive force in German syntax and their structure is actually so regular that they provide a very handy tool to teach the language as a second language.

One other point that is missed in discussions of German is the fact that the German article is more than just a particle marking definiteness—it is basically a function marker in itself, indicating the case of the noun as well. Thus in (12) the article der indicates that the following noun is the subject

of the sentence, whereas the article den indicates that the following noun is the direct object.

- (12) Der unglückliche Mann hat den Wagen zu spät gesehen.
'The unlucky man saw the car too late.'

Once this situation is recognized, then the bracketing system begins to take on a definite pattern: in each case the first bracket in the construction serves as a function marker for the structure that follows, as is shown in the sentences repeated in (13).

- (13)a. weil ich noch mit dem Lehrer sprechen will.
b. Ich weiß nicht, ob die Frau mich kennt.
'I don't know, if the woman knows me'
c. auf den Berg hinauf
d. der große, rothaarige Mann

In (13a) weil indicates that the following clause is a causative. In (13b) ob indicates that the clause is an indirect question. In (13c) the preposition marks a locative adverbial with hinauf adding the information of direction. In (13d) the article der indicates the function of the following NP.

Once we have recognized the function of the first bracket as being to serve as a function marker for a following clause, then it allows a new insight into the role of the inflected verb as the first bracket of the main clause. The inflected verb marks the independent clause. It is, in other words, a function marker in the same sense that a preposition or a conjunction is a function marker, and as such it is ordered at the front of the clause that it is marking. As in the VSO languages, the topic of the sentence precedes the verb, which serves simultaneously as topic marker. This syntactic role of the inflected verb as independent clause marker and topic marker serves to keep the inflected verb at the front of the sentence despite the basically OV which dominates otherwise.

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ASPECTS OF TEMPORAL CLAUSES IN HOPI

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0. Introduction

This paper discusses certain aspects of interclausal relations in Hopi, a Uto-Aztecan language of northern Arizona. Specifically, it focuses on what are called "temporal clauses" in Kalectaca (1978) and "modes" in Whorf (1938). Table I presents the data to be considered. As seen in these examples, Hopi marks

Table I: Data on Hopi temporal clauses.

- (a) um nös-t hiiko
2-SG ate(SG)-PRE(SS) drank(SG)
'After eating, you drank.'
 - (b) um nös-q itam hikya
2-SG ate(SG)-PRE(DS) 1-PL drank(PL)
'After you ate, we drank.'
 - (c) itam nööñös-t hikya
1-PL ate(PL)-PRE(SS) drank(PL)
'After eating, we drank.'
 - (d) itam nööñös-q um hiiko
1-PL ate(PL)-PRE(DS) 2-SG drank(SG)
'After we ate, you drank.'
 - (e) nu' tawlaw-kyang wunima
1-SG sang(SG)-SIM(SS) danced(SG)
'While singing, I danced.'
 - (f) nu' tawlaw-q puma tiiva
1-SG sang(SG)-SIM(DS) 3-PL danced(PL)
'While I sang, they danced.'
 - (g) puma tawlalwa-kyang tiiva
3-PL sang(PL)-SIM(SS) danced(PL)
'While singing, they danced.'
 - (h) puma tawlalwa-q nu' wunima
3-PL sang(PL)-SIM(DS) 1-SG danced(SG)
'While they sang, I danced.'
- | | |
|------------------|----------------------------|
| SG: singular | DS: different subject |
| PL: plural | PRE: precedes temporally |
| SS: same subject | SIM: occurs simultaneously |

temporal clauses for both time sequence and same or different subject with respect to the subject of the main clause; this type of subject marking is often referred to as "switch-reference."¹

In (a) and (c), the marker *-t* indicates that the event associated with it preceded the event in the main clause and also that both events have the same subject. In (b) and (d), the marker *-q* indicates that the event in the temporal clause preceded the event in the main clause but that the subjects of the two clauses are different. Sentences (e) and (g) illustrate the use of *-kyang* to indicate the simultaneity of two events which share the same subject. Finally, (f) and (h) indicate that the ending *-q* can also indicate simultaneity between events which have different subjects.

In his description of verbal categories in Hopi, Whorf (1938) discussed these endings, giving the name "sequential mode" to that interclausal relation marked by *-t*, "consecutive" to that marked by *-kyang* (his *kan*), and "transrelative" to that marked by *-q*. He evidently did not realize, however, that the translations of sentences given to illustrate the transrelative mode actually involved meanings similar to those used to translate the consecutive and sequential modes. In addition, he does not seem to have been aware that the use of the transrelative mode is restricted to clauses in which the subjects differ.

These facts are recognized by Kalectacà (1978: 150), however, where *-q* is viewed as meaning both "preceding" and "simultaneous" when the subjects of the two clauses differ; in the stratificational analysis presented here, the lexeme */-q/* is a syncretization of the sememes */PRE/* 'precedes temporally' and */SIM/* 'occurs simultaneously' under conditions to be specified below. That *-q* is a realization of both */PRE/* and */SIM/* can be demonstrated by the use of certain adverbs which disambiguate the two meanings; cf. (1) and (2):

- (1) puma tawlalwa-q pu' nu' wunima
 3-PL sang(PL)-PRE(DS) then 1-SG danced(SG)
 'After they sang, I danced.'
- (2) puma tawlalwa-q nu' naat wunima
 3-PL sang(PL)-SIM(DS) 1-SG just now danced(SG)
 'While they sang, I danced.'

Table II on the next page shows that there is some freedom in the exact lexotactic positioning of temporal clauses. In sentences where the subject of both clauses is the same, the subject pronoun is realized in the first clause, whether that clause contains a verb marked for temporal relationship or not. Where the subjects are different, each is realized in the subject position of its own clause. Verbs marked with *-t* or *-q* can occur before or after the main clause, but as shown in (o) and (p), a verb marked with *-kyang* can only occur preceding the main verb.²

At this point, our examination of the data is complete, and we may move on to a discussion of the semotactic relationships underlying the surface forms.

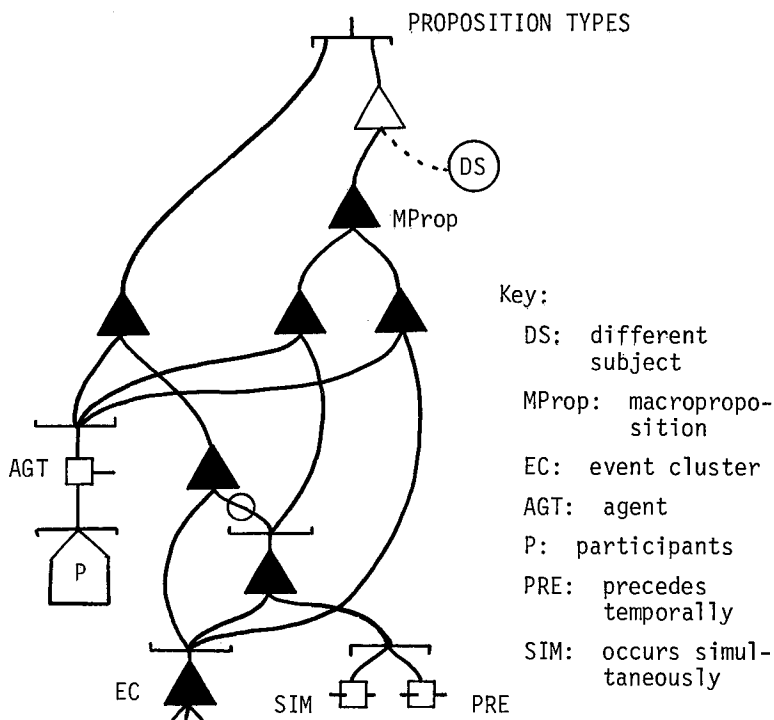
Table II: Variations in the lexotactic positioning of temporal clauses (temporal clauses/verbs are underlined).

- (i) um nös-t hiikö 'After eating, you drank.'
 (j) um hiikö nös-t'a 'You drank after eating.'
 (k) um nös-q itam hikya 'After you ate, we drank.'
 (l) itam hikya um nös-q'ö 'We drank after you ate.'
 (m) puma tawlalwa-q nu' wunima 'While they sang, I danced.'
 (n) nu' wunima puma tawlalwa-q'ö 'I danced while they sang.'
 (o) nu' tawlaw-kyang wunima 'I sang while dancing.'
 (p) *nu' wunima tawlal-kyang

1. The Semotactic Structure of Temporal Clauses in Hopi

The semotactic relations hypothesized to underly the data are given in Figure 1. Here we have two proposition types, one underlying same-subject sentences, the other underlying different-subject sentences. The ordered OR at the top of the diagram

Figure 1: The semotactic relationships underlying the temporal clause data.



indicates that same-subject constructions are processed if possible. This orderedness eliminates cases where the same subject is chosen twice in the other proposition type, thus leading to untested sentences such as (3):

- (3) ? nu' tawlaw-q nu' wunima
 1-SG sang-PRE(DS) 1-SG danced(SG)
 'After I sang I danced.'

It may be that the orderedness of this node is superfluous, in which case a simple unordered OR would do.

In any event, the left-hand path from the topmost OR leads to a semotactic construction consisting of a participant which serves as the AGENT, which will surface as the subject. The AGENT is in turn associated with a structure consisting of at least one event cluster (EC)--an event sememe and its associated participants--and optionally a second event cluster which must be marked for a temporal relation to the first. This semotactic configuration accounts for sentences of the type (a), (c), (e), and (g) in Table I and (i), (j), and (o) in Table II.

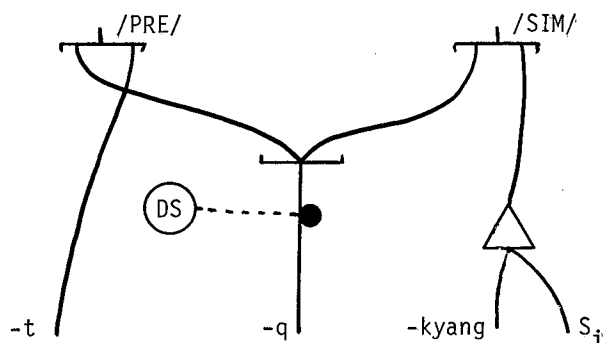
Returning to the topmost OR and following the right-hand path we find a macro-proposition (MProp) consisting of two structures; each of these relates an AGENT participant to an event cluster, with the proviso that one of the event clusters is marked for a temporal relation to the other. This macro-proposition accounts for sentences of the type (b), (d), (f), and (h) in Table I and (k), (l), (m), and (n) in Table II. In addition, the uppermost AND node in this construction provides for the alternate realization of /PRE/ and /SIM/ as -q, via the conditioning line labelled DS, "different subject." The exact effect of this conditioning is the subject of the next section.

2. Semolexic Discrepancies in the Realization of /SIM/ and /PRE/

The semolexic discrepancies needed to account for the proper realization of /PRE/ and /SIM/ and their associated events and participants are (1) the syncretization of /SIM/ and /PRE/ as -q in the presence of DS (as mentioned above, and (2) the correct positioning of temporal clauses within the lexotactics. These two issues are intimately bound up with one another: when a temporal sememe is realized as -q or -t, it's associated clause has a wider range of lexotactic positions. Conversely, when a clause is marked with -kyang, it can only occur as the first clause of the sentence.

Figure 2 on the next page illustrates the relationships necessary to account for these facts. In the presence of DS, both /SIM/ and /PRE/ are realized as -q; otherwise, /PRE/ is realized as -t, and /SIM/ is realized as -kyang plus a marker S_i which ensures that a clause marked with -kyang appears only as the initial clause of the sentence. At this point we can turn to a closer examination of the lexotactic structures underlying the data on Hopi temporal clauses.

Figure 2: The semolexicemic discrepancies for /PRE/ and /SIM/

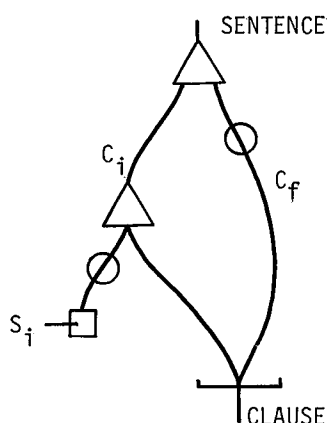


3. The Lexotactic Structures for Hopi Temporal Clauses

In this section we will examine the sentence- and clause-level relationships which are needed to account for the data on temporal clauses. Figure 3 shows the structure of the lexotactics at the rank of SENTENCE. Here, a sentence consists of an obligatory initial clause (C_i) and an optional final clause (C_f).

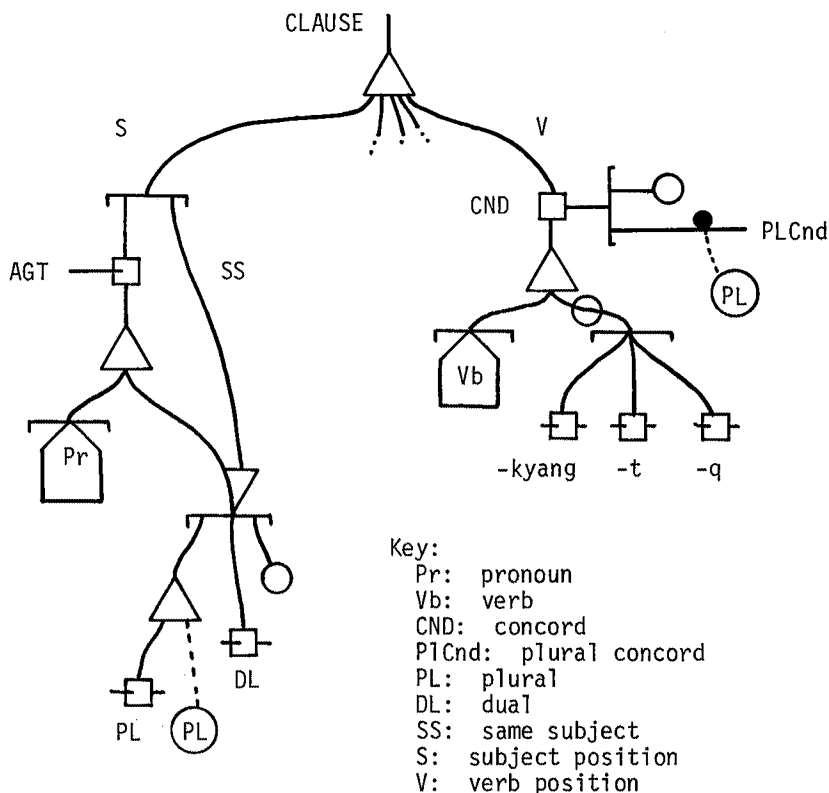
If the verb of a clause is to be marked with -kyang, indicating an event simultaneous with and having the same subject as the event in the main clause, that verb must occur in the initial clause: this is the only sentence position which will accommodate the S_i realization of /SIM/. A verb to be marked with -t, however,³ can occur in either the sentence-initial or -final clause.

Figure 3: The upper lexotactics of Hopi.



The internal structure of the clause is given in Figure 4 on the next page; clause-level relationships other than those of the subject and verb positions are not relevant to the matters under discussion here, and are thus left unspecified. The verb position (V) is related to a concord element (CND) and further to a structure consisting of a verb (Vb) optionally associated with a marker of temporal relationship, -t, -q, or -kyang. Concord on the verb is conditioned by an occurrence of the plural lexeme /PL/ in subject position, verb concord being conditioned via the dotted line labelled PL. If no token of /PL/ appears in the subject

Figure 4: The lexotactic structure of the clause, showing conditioning of verb concord in the absence of an overt subject.



position, the concord element will be realized as zero, resulting in a singular verb.⁴

The fact that the subject position of the clause (S) is related to the realization of AGENT with an ordered OR ensures that there is always an overt subject in the initial clause of the sentence, whether or not this clause is also specified for a temporal relation. Because an agent is always realized in the first available sentence position, however, the subject position in the second clause may be empty, as in the case where the semotactics provides for a single agent associated with two event clusters.

If the final clause of a sentence contains an overt subject, it is this subject which determines the concord on the verb in that clause. When two clauses share the same subject, however, the overt agent will have already been realized as the subject of the sentence-initial clause when verb concord in the second clause must be determined. To provide for the proper conditioning of the second verb, it is necessary to allow for a respecification of

that portion of the previous subject which indicates its grammatical number, since the normal conditioning effects will then follow. This is the function of the line labelled SS ("same subject") which leads from the ordered OR at the subject position of the clause to a REDUPLICATION node just above the specification of grammatical number for an overt subject.⁵ If there is nothing to be realized in the subject position, SS will automatically be taken, thus leading to a reduplication of the number of the subject in the preceding clause, /PL/, /DL/, or zero. If /PL/ occurs, the conditioning line associated with it will ensure plural verb concord; if either /DL/ or zero is reduplicated, no conditioning takes place and the verb is consequently singular. As discussed in Bennett (1980), an occurrence of /PL/ or /DL/ not associated with a noun or pronoun is realized as zero just above the morphotactics, not shown here.

This use of reduplication to condition verb concord in the second, "subjectless," clause takes the place of a "classical" transformational-generative approach involving an overt, co-referential subject in the second clause which conditions concord before being deleted by some sort of equi-NP deletion. The stratificational treatment is felt to be more parsimonious in that it obviates the need for such extra structure and its subsequent deletion by providing in a "subjectless" second clause just that information needed to properly account for verb concord: the number marking of the subject in the preceding clause.

4. Conclusion

In this paper, data concerning temporal clauses in Hopi has been cast into a stratificational model which employs both a semotactics and a lexotactics. This has resulted in what is felt to be a reasonable, straight-forward analysis of (1) the realization of the temporal sememes /SIM/ and /PRE/ in terms of verb markers, switch-reference, and clause-positioning within the sentence, and (2) the conditioning of verb concord in clauses lacking an overt subject. It is hoped, then, that this work contributes to the continuing development of the study of syntax and semantics in both stratificational theory and Hopi linguistics.

NOTES

1. Switch-reference also obtains in Hopi conditional clauses; for details, see Kalectaca (1978: 155-160). Switch-reference occurs in a number of languages in the American Southwest, both Uto-Aztecan and Yuman; thus it begins to take on the status of an areal feature. Switch-reference is also documented for a number of the languages of New Guinea.
2. The endings -t and -q occur as -t'a and -q'ö respectively at the end of an utterance or before a pause; the details of this phenomenon are more appropriately relegated to the phonology and so are not discussed here. Pronoun subjects are used in

all examples merely as a descriptive convenience; full noun phrase subjects follow the same patterns.

3. Comments by William J. Sullivan prompted extensive rethinking of an earlier treatment of the Hopi temporal clause data, leading to the revised analysis presented in this work. It is quite likely, as he pointed out, that discourse notions such as 'given' and 'new' information are relevant to the lexotactic positioning of clauses with *-q* and *-t* with respect to the main clause; regrettably, data which bears on this issue is unavailable for consideration at the present.
4. For a more complete discussion of grammatical number in Hopi and its effect on verb concord, see Bennett (1980).
5. In an earlier treatment of the temporal clause data, SS was directly related to a semotactic position in that construction which accounts for same-subject clauses; a remark by Sydney M. Lamb prompted the revision offered here.

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v

SEMANTICS

FIGURATIVE KICK

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In last year's LACUS paper (Ruhl 1982) I discussed the "literal" uses of the verb kick, those uses involving movement of the leg and foot. My purpose was to show the complex but unified syntax of these uses. I assume in this paper knowledge of that previous paper. I illustrated three major patterns of literal kick (A-C):

- A. Basic pattern: movement of foot and leg: adjuncts indicating direction and goal: I kicked (my foot) (up) (high) (against the fence).
- B. Syntactically shifted pattern: contact with goal: goal as direct object, adjunct indicating place on goal: I kicked the fence (at the top).
- C. A further shifted pattern: movement or change of state in goal or other thing affected by foot and leg movement: adjuncts indicating direction or goal: I kicked the fence over/to pieces.

I argued that patterns (B) and (C) do not require new meanings for kick, but should rather be explained as solely syntactic shifts, with the senses of hitting and secondary movement supplied by inference, the shifts and inference effected by the metonymic relationships between the notions of moving and hitting.

In this paper I want to show how the literal uses of kick relate to "figurative" uses, those not involving foot and leg. My purpose is to exemplify further the definition of FIGURATIVE MEANING which I proposed in my LACUS paper on the noun ice (Ruhl 1982). I assumed, following Bolinger (1965, 567), that most words carry a "something like" potential. These words have a CORE meaning (literal) and PERIPHERAL extensions (figurative), the latter explicable at least in part by reference to the Core. I propose that a word's figurativity be considered as an inherent and necessary part of its identity, not merely as an accidental outgrowth.¹ When applied to single lexical items, figurativity would then refer to the internal organization of a single sense, not a relationship between senses. Specifically, I argue here that the literal and figurative uses of kick are not distinct senses but part of a unified sense.

There is significant overlap of literal and figurative. The three syntactic patterns carry over to the figurative uses: (A-B) in (1-2) and the two variants of (C) in (3-4). (Example sentences are at the end of the paper.) A variant of (C) in the literal pattern has a result-expression as the direct object and this also appears figuratively (5). Also, some sentences can take either a

literal or figurative meaning, with the effective message remaining the same (6-11); while the action indicated can be performed by the foot, it need not be to get the same result.

The Core-Periphery distinction I have made is a Semiotic one, reflecting the difference between arbitrary and non-arbitrary signs. The Core is arbitrary, SYMBOLIC, and thus requires outright learning. The Periphery is non-arbitrary, ICONIC and INDEXICAL, in large part predictable from the Core. Icons and indices are reflected in the figurative processes of METAPHOR and METONYMY. Metaphors are iconic, based on resemblance, similarity. Metonyms are indexical, based on contiguity, juxtaposition. Metaphorically, we can use *kick*, in the basic pattern, to describe an action which resembles that of leg and foot (1). Or, we can use it, in a shifted pattern, to describe an effect of hitting or movement which resembles that of kicking (2-5). Metaphor is "something like."

Metonymy, however, is "something also" (literal) and "something instead" (figurative). In literal examples, metonymy describes something co-occurrent in a situation, a cause to an effect or vice versa, a motive to an act or vice versa, a part to a whole or vice versa. In figurative examples, the co-occurrent part of the message is the real point, with the actually specified part irrelevant and absent. With metonymy as an explanatory device, we can account for the curious indifference of (6-11) to the literal-figurative distinction. Example (6) implies a feeling in the daughters of impatience, boredom and protest, indicated by a nervous, conscious or unconscious act of kicking. The main point of the message is the feeling, not the act; the act itself is not essential. But the act implies the feeling so well that the words describing the act can be used to indicate the feeling, even if the act does not occur. Examples (7-11), when interpreted figuratively, mix metaphor and metonymy. An act and its result are metonymically related. The main point of (7-11) is the result, not the act, and so any act that resembles kicking can be described metaphorically with *kick*. In addition, kicking a person or dog implies disrespectful treatment of them, and so in (7-8) *kick* can be used metonymically to convey disrespect even if no kicking or even any action resembling kicking occurs. As these examples show, the Peripheral meaning of a word further develops the semantic potential of the Core while denying the referential states that the words describe when taken literally. That is the nature of figurative language and the means by which the Core of words can change over time.

The problem in semantic research of figurative language is that of determining the Periphery of a word. The nature of this problem and the possible benefits accruing from it have been obscured by inadequate theory. Although icons and indices are not arbitrary, they are not fully predictable either and must also be learned to some extent. That is, they are not necessarily natural, automatic, and universal. The elusive middle ground of partial predictability is missed by theoreticians who set All-or-Nothing standards. These require that a linguistic entity have a

nature which can be fully deduced by independent entities and principles or else be considered arbitrary. As I have argued elsewhere, the All-or-Nothing standard on the phrasal level is the COMPOSITIONAL ASSUMPTION, which requires that the meaning of a phrase be a sum of its meaningful parts or otherwise be idiomatic. By such standards, iconic and indexical meaning must be considered fully arbitrary also. While researchers have always felt that figurative meaning is somehow related to literal, the non-obviousness of the relationship has deterred any seriously detailed and long-range scholarly enterprise. It has usually been convenient in semantic theories to follow the lead of dictionaries and list all uses of a word as distinct and unrelated.

A fully adequate definition of *kick* thus requires an accounting of its metaphoric and metonymic potentials. Metaphor and metonymy interact in various ways, so it is necessary to choose the simplest circumstances in order to differentiate them as much as possible. Anything human has considerable metonymic potential, so we need to consider examples without human agents or human goals (12-27) in order to determine the metaphoric potential. Kicking is a large scale, all-or-nothing act, lacking the distinguishing finer human traits of subtlety and delicacy, so it can easily be adapted to non-human circumstances. On the evidence of dictionary definitions, we can describe *kick's* metaphoric agency as 'vigorous' and 'forceful', its movements as 'jerky' and 'erratic', and its effect on a goal as 'striking' and 'decisive'. By discarding the requirement of foot and leg, figurative examples conflate the distinction of foot-movement and goal-movement. Thus, while some examples may appear to stress agency (12), the usual dictionary treatment is to consider intransitive objects (12-21) reacting AS IF KICKED, rather than actually doing the kicking, a circumstance fairly clear in (20-21).² Without human agency, the message in transitive examples (22-27) also appears to be concentrated on the goal.

The metonymic potential is more apparent with examples involving humans. As a human act, kicking represents certain qualities of action which it always or typically displays. In literal examples (28-40), *kick* indicates simply being alive and doing something (28), even if only for the sake of doing (29). More specifically, and in tune with the metaphoric potential, *kick* indicates acts which are sudden and decisive (30-31), lacking in fine tuning and thus crude and clumsy (32-33), and displaying negative feelings of satisfaction (34-35), opposition (36-38), and anger (39). In figurative examples the general quality of being alive becomes an indicator of liveliness (40-41), effectiveness (42-43), and simply currency, usually with around and about (44-47). The sudden, decisive quality can imply tough-mindedness and no-nonsense (48-51). The crudeness and clumsiness can imply failure (52). The negative qualities represent a range of beligerent states (53-60), with row or fuss serving as result expressions (61-62). Anger indicating reproach or punishment can be expressed with some kind of conditional statement (63-65), but it can also be fully figurative (66); such reproach can be addressed

to oneself (67-68). When a human is a goal or closely associated with a goal, the verb kick can indicate treatment which is casual (69), rough (70), dehumanizing (72), inconsiderate (73), disgraceful (74), and rejecting (75). The expression kick upstairs (76) gives the rejection an ironic twist. Kick the habit and related expressions (77-80) also indicate rejection.

The quick, no-nonsense, All-or-Nothing quality of the act also is prominent in other examples where it may not be as obvious (81-84). The basketball examples (83-84) have the hands as the instruments, but the stress is not on them but the decisive suddenness of the action. All-or-Nothing lends itself very easily to any mechanical gadget which can be either on or off (85-86); any gadget requiring a more varied and delicate setting would be inappropriate. I suspect that a similar quality is implied in kick in and kick back examples also (87-91); the stress is on the completion of the action, not on any subtlety involved in it. In short, kick describes an action which is seen as clear-cut, requiring or allowing no deliberation. It lends itself with off to certain types of beginnings, such as those in (92-97); even though kicking is literally part of (92) and possibly (96), it is the intended crisp, unfuzzy start which makes kick appropriate.

These general figurative conditions are accompanied by a number of special cases. Although feelings associated with kick are usually negative, it is possible to get 'happiness' as in (98-99); (98) is probably intended literally, (99) figuratively. The literal action implies the feeling and so can figuratively indicate it. Examples (100-104) are a sampling of larger scale figures containing kick; the figure here extends to the phrase or even idea (104), with kick being fully appropriate in the literal basis for the figure. Also, some figurative effects can be located elsewhere than in kick: (105) shows a slight modification of the instrument involved, with (106-108) indicating more modification; in these three cases, the instrument is being compared to a foot. Examples (109-11) indicate personifications. While showing part of the figurative area that kick claims, all of these examples are too specialized to reveal general characteristics.

Kick requires a more extensive treatment than I have given it here. It is part of a long-range project including associated words like slap and lick. Here I have attempted only to sketch the general conclusions toward which I am heading and some of their justifications. One small matter remains worth noting: kick the bucket and other expressions which convey the meaning of 'die' (112-116). I have argued in the past that kick the bucket is not an idiom because there is more than one expression meaning 'die' which includes kick; we can say, AT LEAST, that there is a second kick in the language which has some meaning associated with death. Thus, it seems to me conclusive that these expressions are not idioms. The only question concerns whether this kick can be related to the other one. Examples such as (117) suggest that kick describes a type of death in which the person dying makes a kick, literally or figuratively, as a protesting action, some last

gasp of life. A circumstance like this would metonymically link it and the other expressions to the other kick as well. In closing, I return to my earlier comments and ask All-or-Nothing theoreticians to please recognize the subtlety of language and stop kicking it in the teeth.

FOOTNOTES

¹Words like and and the, of course, are so general in meaning that no figurative meaning is possible. In ongoing research, I show that the verbs break and take are also so general in meaning that they are always literal. However, ALL words can occur in larger expressions which have figurative meaning.

²I suspect that by conflating the two movements, the Periphery of kick creates a pressure, as yet fairly minimal, which could make it eventually a transitive-intransitive verb like break. That is, I kicked the ball would relate to The ball kicked in the same way that I broke the window relates to The window broke.

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1. As the time wore on the sea began kicking up...(Lowell Thomas)
2. The wind still moaned, kicking the snow with its passage. (Al Cody)
3. The war in Europe was kicking prices up. (John Dos Passos)
4. Religion in our Christian land was mostly puerile fiddle-faddle before science kicked it apart. (Philip Wylie)
5. ...the bullets kicked up sprays of snow ahead of me. (Derek Lambert)
6. "Think of your daughters, left to kick their heels in the carriage." (Georgette Heyer)
7. "I'm sure you all saw Dennis Morgan kicking George Raft out upstairs as you came in..." (Mark Goodman)
8. The astounded dog finds himself kicked into the damp outdoors. (Jesse Hill Ford)
9. ...they kicked open the safety valve... (Richard McKenna)
10. ...the Duke kicked on the jet brakes? (Frank Herbert)
11. He poured himself a little tequilla into a mug, drank it off, kicked off the light, and went to sleep. (Olivia Davis)
12. The bomber's engines kick with a 350,000-pound thrust. (Webster's Third)
13. The gun kicked in Matt's hand... (Andre Dubus)
14. ...it [=motor] would kick back against compression and begin reversing. (E. B. White)
15. His stomach kicked. (John Updike)
16. ...his heart would kick against his chest... (Philip Roth)
17. The jumping jack kicked about on the floor until it ran down. (Webster's Third)
18. an engine that kicked a good deal... (Webster's Third)
19. ...the big ship [=plane] under your grip already bucks and kicks in the turbulent dark. (Fleming MacLiesh)
20. MacBean watched the dust kicking around the ports as the machine guns fired. (Oakley Hill)
21. The rifle fired, and the bolt kicked free... (Bruce Palmer)
22. ...you can see shot charges kicking up the dust on beyond the target. (Nash Buckingham)
23. He took the rifle by the muzzle and flung it, and they watched it cartwheel slowly in the air and kick up snow 30 feet away. (Charles Fox)
24. A bullet kicked adobe dust from the wall where Birdwell was setting up the machine gun. (Oakley Hall)
25. ...this might have caused a surge of gas to kick the [perforating] gun up to the top of the lubricator. (Wallace Stegner)
26. This injection tube kicks the protons produced in generator into the actual accelerator. (Smithsonian Magazine)
27. "We don't know if comets formed in the cloud [=Oort Cloud] or formed in the solar system and were kicked out to the cloud by the giant planets," Yeomans said. (Thomas O'Toole)
28. "My mother says I started kicking inside her when Hiroshima happened." (Mary McCarthy)
29. She herself, when she was a girl, used to kick a stone home from school. (Gertrude Friedberg)

30. He kicked back his chair and leaped upon me... (John Barth)
31. Ryan kicked the door open and stepped inside... (David Westheimer)
32. They were kicking aside some trash in the attic... (Robie Macauley)
33. ...someone might kick a wire loose in the semidarkness. (John P. Wiley Jr.)
34. In the corner the baby kicked and cried... (Joanne Greenberg)
35. ...she went dignified up to her room and lay on the bed and brooded, and kicked pillows around. (Wallace Stegner)
36. Danny tried to kick against them. (Paul Horgan)
37. ...Sidacai tried to kick him away... (Cecelia Holland)
38. The major-domo and portiere caught up with him and dragged him, kicking and struggling, back to the hotel. (Bernard Malamud)
39. ...he heard her kick the refrigerator and then the dishwasher. "I hate you broken-down, fucking, second-rate appliances!" she shouted. (John Cheever)
40. "Well, how's old Marsh?" Nathan says. Mr. Marsh owns the drugstore I work in. "Still kicking around?" / "Still kicking." / "He must be eighty-five, or something." (Joyce Carol Oates)
41. "You kicking?" / "Still." / "But not too high, huh?" (Reynolds Price)
42. "Call me back after this ballot, if you're still alive and kicking." / "We'll be alive and kicking so hard nobody will have to tell you." (Tom Wicker)
43. ...an active and kicking liberal wing of the Democratic Party. (Roger Mudd)
44. So we kicked that idea around for a while. (David Eddings)
45. an old chair that kicked around the house for years. (Webster's Third)
46. He's kicked about a good deal before settling down. (Random House)
47. It was the tale trying to take form that kicked about inside the tale-teller at night when he wanted to sleep. (Sherwood Anderson)
48. ...to remind us and to kick the crutch from our complacency. (Hannah Green)
49. For thirty minutes I sat before the typewriter, battling inside myself, trying to kick the panic away... (Les Whitten)
50. ...the tough Marine who has vowed to kick the corps into better shape. (George C. Wilson)
51. The story was kicked out -- without regrets. (Eric Frank Russell)
52. "I hate to say it, Woody, but you kicked this one..." (Gerald Green)
53. "...I can depend on him not to kick up at the wrong time." (Mildred Savage)
54. ...with this Berlin Blockade and the Commies kicking up, you know perfectly well we're going to be in another war. (Otis Carney)

55. kicking at neighboring countries to distract their own people from internal problems. (Webster's Third)
56. For in childhood this great disciplinarian had kicked against all discipline... (Edward Crankshaw)
57. "Then, when he gets your bill, he will not misunderstand or kick." (Sinclair Lewis)
58. tends to kick against authority. (Webster's Third)
59. ...the waiters kicked at the rule. (Funk & Wagnall's)
60. The ham spread was a bit watery but nobody kicked. (John Sayles)
61. The two dogs, left locked up in the apartment, kicked up a tremendous row. (Random House)
62. "Well, the old girl found out, and kicked up a big fuss." (J. G. Ballard)
63. "I could have kicked her head in." (Philip Roth)
64. "I feel like kicking them in the face..." (D. H. Lawrence)
65. "I told him I'd kick him in the backside from here to Haydon if he doesn't come..." (Catherine Cookson)
66. You don't kick people like Sheppard in the butt without them trying to kick you back twice as hard. (Alan Dean Foster)
67. He could have kicked himself. (Francis Clifford)
68. He kicked himself all the way into London for being a coward. (Irving Stone)
69. His case has been kicked around the press as a political football. (Stanley Kauffman)
70. ...he felt compassion for these black innocents so violently kicked into freedom. (Elizabeth Spencer)
71. Their faces as the bullets kicked them to one side were surprised... (Derek Lambert)
72. ...his [=Washington] ragtag army had been kicked all the way across the state by Lord Cornwallis. (Jim Hartz)
73. Business was so good he felt he could kick the customers around a little. (Webster's Third)
74. ...he got kicked off the team for smoking. (Fred Setterberg)
75. took to drink after being kicked in favor of a rival. (Webster's Third)
76. Of course, he'd been kicked upstairs after the fracas in Cairo... (Joseph Hone)
77. But how do we get children to kick the failure habit? (John Holt)
78. The addict thrown into a cell and left to kick the drug cold-turkey... (Ellery Queen)
79. "We've got to kick this addiction." (John McPhee)
80. Ex-Blue Devil Kicks Cancer, Saves Career. (Durham Morning Herald)
81. kicked his car into the lead. (Newsweek)
82. Houston kicked its lead back to 20-13... (Larry McMillen)
83. [Basketball:] I kicked in jumpers, pushed in hooks, swept in shovel shots. (Bob Levin)
84. [Basketball:] "Our job is to give them just one shot and then get the rebound," Jones said. "That's our whole game, to get the ball and kick it out." (AP)

85. When the burner kicks on, warm air rises in the furnace flue by convection. (W. Clyde Lamney)
86. ...half the circularity fused, alarm bells went off, the sprinkler system and a couple of CO2 cylinders kicked in... (Thomas Pynchon)
87. "He kicked in big for Hunt's campaign." (Tom Wicker)
88. asked to kick a dollar in for a present for the boss. (Webster's Third)
89. forced to kick back out of every paycheck. (Webster's Third)
90. The baseball strike was 12 days old yesterday, but today, when it hits 13, the owners' considerable strike insurance kicks in. (San Francisco Chronicle)
91. "You don't turn things around in your farm system overnight," Kennedy said. "Ours, in fact, didn't really start to kick in until last season..." (Joe Goddard)
92. The Giants won the toss and elected to kick off. (Random House)
93. For January kicks off the new year's sport and outdoors vacation show season across the South. (Southern Living)
94. In October, events kick off with Leif Erickson Day in Jensen Beach. (Bill Clark)
95. The campaign kicked off at Chicago in July... (Carolyn Bennett Patterson)
96. I kicked off, and the canoe slid into the darkness. (Jerry Kambites)
97. [Driving:] ...we kicked off into the windy, blue space of the Golden Gate. (Janus Crumley)
98. He stood in the path and lifted his spread hand high and brought it down again and again in his laughter. He kicked up his heels. (Eudora Welty)
99. It [=moonshine] will make you kick your heels high. (Jesse Stuart)
100. The lawyers continue to kick up dust around the regional water crisis. (Virginian-Pilot)
101. "Well, you sure kicked over a bucket of worms with that one," (Ben Haas)
102. "It'll kick over a dozen applecarts, but it's necessary." (Margery Allingham)
103. "Don't kick a dead horse," said Burt. (Ellery Queen)
104. He's tired of being the 98-pound weakling who gets sand kicked in his face. (Ira Miller)
105. ...I tried dribbling through two guys and ended up kicking the ball out of bounds off my knee. (Bob Levin)
106. ...Father Burner anointed the air and shot a whisper, "God Bless you," kicking the window shut with the heel of his hand. (J. L. Powers)
107. ...he lapsed into what seemed certain sleep. / But in less than a minute his chin kicked the air... (Reynolds Price)
108. ...Macalister's boy had caught a mackerel, and it lay kicking on the floor... (Virginia Woolf)
109. ...[it] is an idea that has been kicked in the head. (William Safire)

110. ...Fate began drawing back her leg to kick him. (Flannery O'Connor)
111. Kicking its white-water heels through sparsely populated hills as tough and worn as a farmer's overalls, this frisky calf of a stream... (Harvey Arden)
112. "Do you realize how fast people can kick the bucket?" (Daphne Athas)
113. "Did you know, he has a rheumatic heart? He could kick off at any minute." (John Updike)
114. He kicked in after a long and painful illness. (Random House)
115. "Isn't your father going to dump the lot on the Daltons when he kicks it?" (Tabitha King)
116. "I wanna make a will before I kick." (Consumer Survival Kit)
117. The shotgun caught Norm Lilly full in the chest. Lilly kicked once and died. (Larry Niven and Jerry Pournelle)

APPEAR AND SEEM -- LEXICAL INTEGRITY IN DANGER?

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Introduction

In an article arguing against the commonly accepted claim among transformationalists that the deep structure of "It seems likely that John has taken them" is "That John has taken them seems likely", Aimo Seppänen asserts that there are at least two verbs appear, appear₁ and appear₂. He argues that appear₁ is synonymous with seem on the basis of their syntactic identicalness: "It seems/appears that John has taken them", which through Raising and Extraposition can be transformed into "John seems/appears to have taken them". Appear₂, meaning roughly 'be clear, be manifest', is syntactically and semantically distinct from appear₁, Seppänen says, and he goes so far as to claim that the two verbs appear are homonyms. He shows that appear₂ can occur in the following syntactic frames, rejected by seem (and appear₁): "(a) non-sentential subject: These facts appear clearly from the new documents now made available; (b) non-extraposed sentential subject (that or wh-) "That they were innocent appeared clearly from the investigation"; (c) "Emphatic extraposition": That did not appear from his application that he had received first class honours ; (d) left dislocation: That they were not members of our association -- that appeared to everyone from the first examination of the documents ; (e) co-ordination with another clause: That the boys were innocent appeared clearly to the investigators and was made known through the help of the local press. "The primary aim of this paper is to see if a deeper syntagmatic investigation, applying a close collocational analysis, is capable of proving or disproving Seppänen's claims. I have claimed in my earlier LACUS papers, following Bolinger and Ruhl, that each lexical item is intact, i.e. it cannot be replaced with another word without some alteration of the meaning. A consequence of the claim "one form, one meaning", which I am thus an adherent of, is that words should be regarded to have one basic meaning and the common polysemic treatment words get in dictionaries and in linguistic writing is misleading. A survey of my material, which is not at all extensive enough (thousands of examples should form the basis of safe conclusions), seems to indicate that a "third" appear, meaning 'emerge' ("He appeared in the room"), carries the fundamental meaning and that the other two "senses" constitute psychologically well-based extensions of this semantic core that should profitably be treated as two references rather than two senses or even as two homonyms.

Collocational similarities between seem and appear

The collocational patterns of seem and appear coincide to a conspicuously great extent: both occur with an adjectival complement without to be (examples (1) and (14) at the end of the paper). There is, however, a significant difference between them in that roughly 70 per cent with seem evince that pattern, whereas with appear, this pattern occurs only in 35 per cent of the cases when an adjective is the complement (see examples (58) and (148) with a full "to be + adj." pattern). Similar collocations for both verbs are exemplified by (9) and (14), (112) and (142), (151) and (197), (203) and (224), (260) and (249), (268) and (269).

It is noteworthy, however, that seem alone collocates with never and always (see (280)), now (see (293)), and as (see (331)) and that it dominates markedly in collocation with for a moment. These collocational similarities point at a near-identicalness as to the semantic status of the two verbs, but if we penetrate deeper in our collocational analysis, differences will emerge.

The intentionality of appear

Seem could hardly replace appear in examples (1) - (13), since all evidence shows that only appear, suggesting here 'create an impression', subsumes behavioural features that are open to choice. Most often such a choice seems to be forced, to take place under duress, as will be seen from example (4), where the suggestion is that social difficulties cause an urgent need to create an impression of balance and sociability. Thus, if the effort is successful, the result will be "appear balanced and sociable, but actually have great social difficulties".

Examples (2) - (13) can be similarly rephrased, showing that the collocational type demonstrated by examples (56) - (61) reflect an essential semantic property of appear. However, (1) shows us that this implication of appear is not obligatory, unless the speaker is suggesting that the demonstrated concern is only a form of pretence with the implication "but actually it is a shammed emotion you are displaying". What is particularly important to note here is that the intentionality implies a high degree of exactness as to what behavioural features underlie appear: if you want to create an impression of sociability, and is making an effort to do so, you are aware of what kind of behaviour you should resort to. Thus, there is a specificity and concreteness about appear that seem lacks. This is manifested in (6) (seem to bite) and in the examples with the collocation make...seem, where the complements of seem are more diffuse than those of make... appear: it is perfectly obvious what exactly underlies be elsewhere ((12)), attractive ((10)), whereas wise ((14)), for example, is more abstract and diffuse. To heighten communicative efficiency, the abstract concepts appearing as complements of seem materialize in metaphorical terms, bite, explode ((15)), and relaxed ((16)).

Examples (20) - (23) corroborate the intentionality of appear in a somewhat stronger form; an effort is scaled up to a determination

((20)), and in (21) and (22), appear collocates with verbs expressing obligation. Can in (23) brings out the optionality of appear: three options are at their disposal to make certain people believe that the narrator leaves Venice of his own accord.

The fundamental meaning of appear

Examples (24) - (27) are extremely important for my central claim in this paper in that appear is used here in its fundamental meaning 'become visible', 'show up', 'emerge'. Of course, this is most obvious in (24), where appear gets the "reading" 'become visible', but it is important to note that the emergence which is thus registered is immediately followed by a characterization of the appearance: they appeared and they were soft-footed. The inbred human tendency to assign a property to the phenomenon with which one is perceptually confronted is the psychological basis of the semantic qualities of both appear and seem, as we shall see below. The collocation appear as is highly significant for my main claims since appear can show up as a "factive" as well as a "non-factive" verb. It is of course "non-factive" in (3), (26) and (27). In the latter two examples, the interpretation of as is "appear in the guise of". However, in spite of the patent subjectiveness, (25) approaches "factiveness": "from the way I viewed him he appeared to me to be a complete and successful human being". Compare Seppänen's example (e) cited above, which might be rewritten "The boy appeared as guilty to the investigators". No matter how clearly and conclusively the guilt might have been established, it is virtually inescapable to be guided in such a judgment at least to some extent by slanted prejudiced ideas. (25) can easily fit in in the syntactic frames which are set up by Seppänen for appear₂, and yet, since appear is not completely "factive", it seems to fulfill the conditions for being an example of appear₁ as well. It is impossible to make a clear-cut distinction between Seppänen's appear₁ and appear₂. In a sense appear is always "factive" since it has a physical implication, 'emerge', that is lacking in seem. This "factiveness" underlies the collocation appear as, where as represents the interpretation that is applied to appear (= 'emerge'). It should also be clear that the specificity and concreteness of appear is based on this "factiveness". For example, in "He tried to appear calm", the pretence implies an actual emergence: he actually emerges as a calm person in adopting the behavioural features that are linked with such a quality.

The verisimilitude of appear

Examples (28) - (55) show how firmly entrenched an appearance is in the mind as corresponding with a true state of affairs, but such an impression is highly deceptive because it is resting on a quick snapshot impression, which thus cannot escape a certain superficiality. See (53), where we can also note the visual basis of appear ("stare"). (28), where one perception releases another, stresses the factuality of appear: first the American vehicles appear, then some vehicles appear as French, after which some

emerge looking as if they were Japanese. Appear is in the middle of a scale of a decreasing likelihood. Various contextual cues indicate here that there is a certain visual basis for appear: "mouths full" ((29)), "paint" (a registration of visual impressions) ((30)), "to the bright sunny street" ((31)), "was entertaining..." (he could be perceived) ((32)); in (33), an expected state paves the way for the appearance of one perception being selected at the expense of others. In (34), the fact that he "rouses himself almost apathetically" ensures that the appearance is firmly entrenched. In (36) through (38), the registration of several qualities lends credibility to the appearances. In (39), the last sentence forms a firm secure basis on which the appearance rests. (40), appearing in linguistic writing, has an argumentative function,¹⁾ and in (41), we are dealing with a similar case: other people have tried to convince the narrator of the necessity of "the spider web of facts". In (42), Conway alone knows what underlies "something", which gives him exact knowledge what behavioural features underlie appear. (43) may be interpreted: every single piece appears (=emerges) in a broken state, thus emphasizing the decay and squalor. In (44) and (45), a conscientious scrutiny forms the basis of appear. In (46) through (51), the contextual cues lending a very high degree of credibility to appear are underlined. In (52), the point of vantage on a bridge above the observed highway guarantees a high degree of visuality in the basis of appear.

(53) may be regarded as a parallel of (66), but it is interesting because of the usual high degree of verisimilitude that characterizes appear; seem, which would perhaps be somewhat unnatural in the context preceding the verb here, would certainly not be followed by "rushed..." Seem would imply that the person made an impression of leaping out of the window, perhaps going through the motions of this act, but the observer would remain assured that the person remained standing, and the received impression would not occasion the desperate rush to the window. In (54) and (55), without appearing... is tantamount with "pretended not...", it being a certain fact that a perceptual registration takes place. In (57) through (70), the collocation with but (actually/really)... being highly significant as stated above, the deceptive appearance is seen through and a penetration to reality occurs. An exception is of course (67); the appearance is for once the appearance of truth.

The elaborateness of seem

Example (70) and (71) through (95) should be regarded as being illustrative of the "adjectival" elaborateness of seem contrasted with the snapshot impressionistic "verbal" character of appear. In (70), the perspicacious observer penetrates the superficial appearances in order that he might register a more deep-seated and permanent characteristic of the observed person. Appear could in fact be inserted here to be followed by its characteristic sequel but...: "He appeared an unimpressive figure, with his arm in a sling..., but actually he seemed completely self-possessed." (71) through (96) show that the sequel and... is a significant colloca-

tion of seem, even more so than but... visavis appear, and it is a safe assumption that it is an important clue to the semantics of seem. As concentration here must be on appear, space cannot be used for a description of the collocational particulars of seem. Suffice it to say that the and... sequel in (73) and (74) expresses a consequence of the impression expressed by seem, whereas in (81), for example, the sequel functions in part as the basis of the impression. In (86), "not perturbed" constitutes an integrated component of the personality of which "enjoy seeing himself" is a manifestation. Thus, "enjoy..." is invested with a deeper significance than it has if it is taken merely at its face value. This is typical of seem because this verb expresses a more thoroughly processed perception than appear.

"Appear₁" and "appear₂" merging in the same form

Appeared in (97) and (98) is evidently "factive"; it can be paraphrased 'be clear'/'be obvious' (these paraphrases, however, are too specific for the deeper, abstract semantic core of appear, being a case of what Ruhl calls "pragmatic specialization"/see Ruhl 1976, for example/), and the verb functions in Seppänen's tests for "appear₂". But in (100), where the complement emerges in the progressive tense, which typically has an interpretative function (see below), the collocation with from... indicates that there is a strong likelihood that the expression in the men's glances should be interpreted as symptoms of a rising vexation, even approaching certainty: "from the glances it was clear that..." However, the interpretative progressive tense implies that the conclusion that is thus drawn from the exchanged looks is merely a tentative one, and the element of uncertainty which is thus present implies that it is impossible to distinguish between "appear₁" and "appear₂". Seppänen's tests apply; thus, for example, "emphatic extraposition": "that appeared from the exchanged looks that they were giving rise to an increasing degree of vexation". It is true that appear cannot be replaced by seem primarily because of the collocation with from... (no such example has been found in my extensive material). But the theses "one form one meaning" and semantic determinism, i. e. there is in every case some features in the context (textual and non-textual) that determine the selection of one form at the expense of a "synonym", imply that appear and seem are not interchangeable in any piece of text. Example (99), although it contains a concealed "from" phrase: "... judging from his musical past", likewise expresses an interpretation rather than an obvious fact. Perhaps the most important example in this context is (101). Seppänen's "appear₂" can obviously not occur in the syntactic frame "Subject + appear to + verb/(to be) adjective". However, in (101) appear, which has here its specialized inferential meaning 'be clear'/'be obvious', occurs in such a frame. The author, knowing that appear, "appear₁" as well as "appear₂", most often has an explicit contextual basis as evidence (Ljung uses this term /p. 49/) for the expressed impression, mistakenly uses "appear₁" instead of letting "appear₂" adopt the position of evidenced

by, appear from thus replacing this verb. Perhaps the underlying reason for this "mistake" is that the author wants to suggest that the owners of the industry were making their industry appear innocuous, whereas actually it was far from innocuous. These examples demonstrate that the transition from "appear₁" to "appear₂" is rather a continuum than a distinct boundary, and in this transition area Seppänen's tests break down. The following examples, (103) through (110), are examples of appear being based on explicit evidence. In (103), for example, "appear₂" could be used, "that he was amicable and relaxed appeared from the fact that he had a carnation etc." But because of the "active" dynamicalness of appear its fundamental meaning being 'emerge', the structure in (103) is applied. Because of this visual aspect of appear, the evidence of the impression which is described is remarkably articulate, much more so than in the case of seem. Perhaps we might say that appear is contextually cohesive "to the left", since the evidence (at least in time, but not always in the context) precedes the created impression. This seems to be linked with the characteristic sequel but... for appear. On the other hand, seem with its characteristic and... sequel is contextually cohesive "to the right".

Since appear has a physical sense, "appear₁" as well as "appear₂" is based on "Something appears". A perception of a phenomenon requires qualification, i. e. "something" tends to be supplied with a property. In "appear₂", this property is immediately apparent. Thus, there is no doubt what "something" in "from the documents it appears he took his degree in 1970" is like. However, in example (99), there is a time lag between the appearance and the application of a qualification, which means that "something" in "something appears" can be characterized only after it has been given time extension. The use of the progressive ("to be giving rise") in (100) is an indication of the fact that an act is interpreted as it is being carried out and is thus also an indication of there being a time lag between the appearance of the phenomenon and the interpretation that is applied to it. Seem, lacking the physical sense 'emerge' of appear, is free from the duality that is thus characteristic of appear and enters the picture at a later stage, purely signifying the application of qualities to an appearance/emergence. In (101), "something appears" is filled, as it were, by "they were smilingly invited". This appearance gets the interpretation, after a certain time-lag, that "the industry was innocuous". In "from the document it appeared that...", the appearance and the qualification merge in that "something" in "something (the document) appears" is qualified at the very moment when the appearance occurs.

In these examples the semantic duality of appear is manifest in that the detailed explicit "concrete" contextual basis constituting the evidence of the impression that appear expresses makes up a primitive appearance, "something appears", on which the more refined "surface" appear is based. Thus, in (104), the underlying structure is "Captain Demerest appeared (=emerged), glancing..., observing..., checking...". These verb forms are the results of an immediate interpretation of the emergence, an interpretation that

reaches a more refined status, after a certain creative time lag, in the "surface" occurrence of appear. We note that this time extension is formally marked in the repetition of that, signifying a certain hedging. In (105), "something appears" is filled with "a certain specified task appears", and a more careful consideration of the specifics of this task leads to the judgement "the task appears impossible". These specific circumstances forming the basis of the appearance may occur in a from-phrase: "from these specific properties of the task the task appears impossible". Here, there is a creative time lag between the consideration of the circumstances and the resulting judgement. Seem is completely free from this tie to an underlying perception and operates unhampered on the time axis that is extended from the initial appearance. "Appear₁" is closer to "appear₂" than to seem because of the duality that marks appear. This duality is a manifestation of the human tendency to attach a property to all perceptions, to qualify all appearances. Appear as it were adopts three different areas of the human processing of perceptions: "he appeared in the room" is the first stage, "it appeared from the document that..." is the second stage, where the appearance of the document is immediately followed by a self-evident qualification of the perception (cf. example (24) where the interpretation soft-footed is virtually simultaneous with the appearance). The third stage, "appear", is represented by cases where the duality of appear is most obvious, i. e. in cases where the "surface" form appear as it were includes its "concrete" basis. Thus, in (103), the form appear includes "something appears", i. e. "speechlessness and motionlessness appear/ he appears speechless and motionless. Thunderstricken has "call"-status: "what appears as speechlessness and motionlessness should be called, after some deliberation, thunderstricken". Owing to its basic physical sense, 'emerge', appear can cooccur with a from-phrase: "from the speechlessness and the motionlessness thunderstricken appears. Seppänen's tests break down here. We can have for example "that appears from... that he was thunderstricken".

Appear and seem collocating with the progressive

Examples (111) through (146) reflect an essential aspect of seem and appear: the complement appears in the progressive. Expressing impressions of phenomena, the two verbs are interpretative in nature, and so is the progressive tense (see Bäcklund 1982, the Conclusion, and Ljung, p. 69 ff.), as in "If I told you this, I would be telling a lie". Because of their interpretativeness, both the progressive and seem/appear cooccur with explicit evidence. (Ljung gives the example /p. 49/ "John seems (to be) angry/John is being angry. He just kicked his dog".) Appear with its physical sense, confers more life on the metaphor. Appear + complement is here a comment on a physical event, "the heart beat faster and faster" until it actually evinced symptoms of "roaring like a generator", but the deceptiveness of this appearance is obvious from the start. Seem + complement is frequently a comment on an abstract concept, seem + complement adding as if introducing a concretization of the abstract concept: See (129), "her sense of reality was giving way",

(135), "move slowly", and (137), "voice was dreamy".

Seem/appear + (to be) + adjective

Examples (147) - (149) are crucial ones, illustrating the difference between seem/appear to be + adj. and seem/appear + adj. The difference consists mainly in a difference in the source of impression (I have got this term from Aimo Seppänen in personal communication). In (147) the impression unaware is backed up by evidence appearing in the context ("he looked neither to left nor to right"), which is a registered perception of the source that is immediately followed by a new perception, partially based on the first perception. In (148), on the other hand, the flow of impressions is abruptly broken by but, which implies that a reassessment is made of the source. Thus, a difference in the time during which perceptions of the source are being processed is the crucial factor: a longer time allotted for registration of properties paves the way for seem + adj.: he seem unaware, whereas a short time requires a full proposition: seem he is unaware. In the former case seem is equivalent with be, whereas in the latter case be occurs in its complement. Puzzlingly, in (149), to be follows appear in spite of the evidence being more elaborate than in (147). The reason for this is probably that the evidence is so obvious, so obtrusive that it immediately and almost automatically releases an interpretation. Perhaps it is the element of surprise at their being unaware of the dreadful cold that underlies the sudden interpretation. The underlined sequel constitutes the concrete basis of appear. Appear, with its fundamental meaning 'emerge', is apt to give such snapshot interpretations of impressions. Appear's fundamental dynamicalness implies that the interpreter has a shorter time at his disposal than if he selects seem. Considering the time difference between "seem/appear to be + adj." and "seem/appear + adj.", "appear + adj." and "seem + to be + adj." constitute contiguous categories, and it is significant that the poles, "appear to be + adj." and "seem + adj.", implying the shortest possible time and the longest possible time for processing a perception, evince the highest frequencies. This supports my claim that seem and appear have clearly discrete senses in that the poles are heaviest as it were.

The interpretative character of seem versus the affirmative factuality of appear

Examples (150) - (250) offer a survey of seem and appear in the syntactic frames "John seems/appears to be tired" and "It seems/appears that John is tired". It will be seen that the complements of seem are generally more imaginative and metaphorical in nature than those of appear. As an example, consider (165). A noteworthy circumstance is the fact that as if- and like- complements operate as supporters of the impressions which are expressed by seem (examples (187), (188), and (189) /a comparative as-clause/), (189) and (190)). The explanation is probably that seem, expressing a more elaborately processed impression, is more tentative in na-

ture than appear, which often embraces an initial impression as gospel truth, as it were. Another point of interest in this section is the collocation seem to indicate ((182)-(185)/show, which is closely related/), which seems to resonate the tentativeness of seem, since indicate means 'give evidence'. Appear, it would seem, requires definiteness in its complement, whereas the complement of seem is more diffuse. A further interesting aspect in this section is the fact that the progressive appears in the that-clause ((203) - (207)). Of particular interest is "is stopping there" ((204)), where the meaning of stop is dislocated or "pried loose" (Ljung's term) from its "ordinary" meaning. We have a parallel in example (111), where the dominant emotion fear twists the woman's perception, thus causing see to be applied in a dislocated sense in the progressive. An interesting consequence of this dislocation is that in "Joan is hearing voices telling her to save France", there is a suggestion that Joan is having hallucinations, whereas in "Joan can hear voices telling her to save France", Joan's credibility is intimated as much higher (see Ljung, p. 87). The use of these verbs in the progressive in the complement of seem testifies to the pronounced interpretativeness of seem.

Examples (222) - (250) offer further evidence as to the affirmative factuality of appear. We shall dwell on three points of interest here. First, notice the collocation of appear with see(ing) ((244)). Seem would have an unnatural ring here. Second, appear in (246) and (247) is rather Seppänen's "appear₂"; it can be paraphrased by turn out. Third, and most importantly, appear can obviously refer to an impression which arises from somebody's utterance: in (225), appear is evidently used in the sense "appear₁", since a certain reply gives rise to a specific interpretation. Example (226), on the other hand, represents a more factual appear: it seems to me it is located in an intermediate position between "appear₁" and "appear₂", since what somebody has uttered cannot be understood as unquestionably representing the truth. This also applies to (243). Finally, again, in (249) appear must be interpreted as "appear₂" because the appearance is stated as knowledge here and because of the many enumerated concrete details. However, this will prove beyond doubt that there is no distinguishing between "appear₁" and "appear₂".

Appear, because of its basic meaning 'emerge', has an affirmative flavour that seem lacks. This can be observed in (230), for example, where a thoroughfare cutting off a street indeed ends it, at least temporarily. Speakers also utilize the affirmative note in appear to achieve certain stylistic effects. Thus, the patent irony (made clear by the context) of (238) would be blunted if seem replaced appear, the hyperbole in (239) would lose strength with seem, and the use of seem for appear in (240) would cause the effective simile to fade.

The "call"-status of seem's complement

As a result of its position further away from the initial appearance of a phenomenon, being pushed a longer way on the time axis

starting with appear = 'emerge', seem is given a freer semantic rein than appear and has thus a more independent semantic status. This is most obvious in collocation with like and as if. Because of its sense 'emerge', appear is here given a much more rigid role than the liberated seem since it cannot, as it were, let like and as if run away with its sober factuality. Appear denotes here a generally verifiable impression,²⁾ whereas seem is more individualistic in that it allows the speaker freedom to clothe even rather bold and highly subjective impressions in words. Compare examples (269) and (270) with (266).

The similarity expressed in the complements of seem moves from more like to less like and is thus given a wider scope than the complement of appear, which must register something that can be objectively seen by anyone, appear meaning 'emerge'. As a consequence, seem allows the speaker linguistic creativity, manifested in an abstractness lacking in the complement of appear and in a tentative groping for the linguistic expression most adequately corresponding with certain individual perceptions. Put somewhat differently, the "call"-status is much more pronounced for the complement of seem than for the complement of appear. It seems that it is in collocation with like that it is most clear that appear retains its sense 'emerge'. Collocation with as-clauses (examples (339) through (340)) is typical of seem (no occurrence of appear in my material), and it is here that the independent status of seem is reflected most clearly, for it seems that the connective as has the power to erase the hypotheticalness that is assigned to seem by if. Consider for example (266), which evinces a drastic linguistic creativity. The underlying semantic configuration may be said to be: "if she had swallowed a pumpkin, seem would have the value that as gives to it". As, which seems to focalize the verb in the corresponding main clause (see Bäcklund, forthcoming), charms away the hypotheticalness that the if-clause gives seem. A consequence of the independent status of seem is its flexibility and adjustability. Thus, it can even occur in the progressive, which is hardly the case with appear (examples (340) and (341)). It seems that the progressiveness of the two acts "was creating ... impression" and "move" (the progressiveness of this verb is emphasized by as, which is very likely to have something to do with seem occurring in the progressive) is transferred to the adjustable seem.

Conclusion

The following two examples, occurring very close to each other in the same context, show that the superficial appear has a "verbal" character whereas seem, expressing more thoroughly processed perceptions, is more "adjectival".

(I) Something strange happened in that instant. As I turned to look at her she seemed transfigured. Divested of her shapeless coat she seemed taller and more dignified. But it was her expression that struck me. She stood there in the doorway, her gaze fixed upon the golden pair by the fire, her head thrown back, her face exceedingly pale; and she appeared

to me for a second like some insolent and powerful captain, returning booted and spurred from a field of triumph, the dust of battle yet upon him, confronting the sovereign powers whom he was now ready if need be to bend to his will. (Iris Murdoch)

(II) Palmer had his arm round my wife and their faces... were seen clearly in profile, each outlined with a pencil of gold. They seemed in that momentary vision of them like deities upon an Indian frieze, enthroned, inhumanly beautiful, a pair of sovereigns, distant and serene. (Iris Murdoch)

First of all we can observe, as we have noted above, that appear has an underlying concrete basis, often very detailed (note the detailed observations here: "she stood there,..., her gaze fixed etc."). Denoting impressions and thus being interpretative in nature, both seem and appear are contextually cohesive; seem, its characteristic sequel and... indicating a seriality of impressions, is cohesive "to the right", whereas appear is cohesive "to the left" as is obvious from (I). This cohesiveness "to the left" of appear is of course linked with its double nature in that the first appearance, "something appears" (equivalent here with the detailed description of the woman, "She stood there." etc.) is included in the "surface" form appear which represents a more refined and "adjectival" stage in the processing of a perception. However, with like as in (I), appear has a cohesiveness "to the right" as well, but this is verbal and evanescent in nature, whereas the cohesiveness of seem is adjectival and more firmly entrenched in lasting concepts (compare the evanescent NP "the dust of battle yet upon him" for appear versus the lasting quality of the NP "a pair of sovereigns" for seem).³⁾ Perhaps we may say that appear with its meaning 'emerge' is a dynamic verb, whereas seem may be regarded as a stative verb. This stativeness might be the reason why seem attracts collocations with time expressions, for a moment (appear also has this collocation, however, see (280) and (I)), never and now ((281) - (311)) in that seem, being neutral to the time aspect in itself, attracts time expressions when these seem to be justified. Never might have been transferred from the verb that occurs in the complement of seem, the frequency of the collocation never seem being another indication of the adjustability of the verb. Never seems to have the function of emphasizing an emotion of a negative nature, like irritation ((281) and (282)). The irritation directs itself against the behaviour that underlies seem as its 'evidence'. From observations of spoken English I have got the impression that the collocation seem to realize is very common in negated contexts. I would guess that the underlying factor is that realize also rests on 'evidence' ("something made me realize"...). The double emphasis on the underlying 'evidence' in (not) seem to realize heightens the note of irritation. Never, of course, screws up this note even more. The fact that seem most often relies on underlying 'evidence' may thus be one of the most important factors determining the collocation of seem with never.

Seem is less concrete and more versatile and adjustable than

seem. The affirmativeness places not after appear in (343), where as the negation occurs before seem in (14), which constitutes a minimal pair with (339), and this affirmativeness is dependent on the meaning of appear being 'emerge' even in those cases where appear strikes the analyst as being synonymous with seem. The last sentence in example (339) illustrates the factuality of appear in that it brings home the appearance of youth as it were.

However, as we are trying to conclude the discussion, we are faced with a paradox: the and... sequel intimates that seem is more creditable, closer to the truth, than some uses of appear, where this verb has an explicit or implicit but... sequel. How can we reconcile these facts with each other? The answer lies in a crucial difference in time. The sense 'emerge', being punctual, has no time extension. But a time axis of course starts at the moment of emergence, and properties are assigned to the appearance. Example (24) marks this significant transition: waiters appeared and were soft-footed. Appear denotes a quick, almost snapshot impression. Because of this snapshot character of appear, the human mind strives for maximum credibility, it tries to back up its impressions with evidence: something appears from document, what somebody has uttered etc., hence the factuality of appear, manifested in various collocations. However, there is no avoiding becoming in a large number of cases the victim of an optical illusion because of a superficially processed perception. Hence, the wide gap between appearance and reality, noted in many examples. Seem "takes over" the time axis and proceeds to the point of becoming a stative verb. This implies that the impressions rest on more thoroughly processed perceptions. The result is a higher degree of verisimilitude (manifested in and... sequels) and the seemingly contradictory observation that seem invites imaginative creative linguistic processes resulting in "call"-words. As the process along the time axis proceeds, there is more time for the use of stable adjectives and for relexicalization of words in the form of new creations, metaphors and other "call"-words. A collocational indication of creativeness is the frequent conjunction of adjectives after seem ((313) - (326)), this being a symptom of a tentative search for the most adequate lexical expression of an impression. This is even more obvious in (327) - (331) (note particularly the as if-clause in (330), heightening the creative "adjectivalness" and a third adjective preceded by even in (331)). Of particular interest is example (340), where the metaphorical uncoil is given more precision after a slight pause.

Because of lack of space, I have not been able to discuss many side-issues, as, for example, the relevance of the adjectives, adverbs, and nouns seeming(ly), apparent(ly), semblance and appearance. What I have primarily tried to show in this paper is that a deeper investigation of syntagmatic properties of a word is required for safe semantic conclusions to be drawn. The collocational approach demonstrated here shows that each word has its own syntax, and the inevitable conclusion is that we should keep respecting the integrity of words.

Notes

- 1) Appear may be more common in non-fiction because of its argumentative function. See for example Ruhl's papers in the various proceedings from LACUS conferences, where appear dominates over seem.
- 2) However, this is not the case in example (I) in the Conclusion.
- 3) It may be argued that the properties following seem like are evanescent as well, denoting dreamlike transitory qualities. But the fact that the complements are adjectives here, whereas in the case of appear like (in (I)) they are verbal forms, justifies my assertion, since adjectives in general have a more lasting nature than verbs.

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- (1) It is charming of you to appear so concerned... (James Hilton)
- (2) "To begin with, I said with as great an appearance of coolness as I could manage, "I don't know Lord Loamby and he certainly knows nothing about me." (Agatha Christie)
- (3) No longer the prosperous determined woman of the world, but a broken nerve creature making an obvious and pathetic effort to appear as normal. (Agatha Christie)
- (4) He found it difficult to get on with other people and had to make a great effort to appear balanced and sociable. (Ken Follett)
- (5) That is what is said of elderly ladies who are trying to appear younger than they are. (Agatha Christie)
- (6) I felt something else; hands that tried to appear gentle, but instead were sharp, hands that seemed to bite into me, rather than support me as they raised me to a sitting position. (Catherine Gaspin)
- (7) Egg was principally concerned with endeavouring to appear as though fifty or sixty pounds was a mere bagatelle to pay for a dress. (Agatha Christie)
- (8) But to be had up like that by Palmer and Antonia, to have the thing thrust at me as a crime, and at the same time stroked and coaxed in their benevolent imagination, was to make it appear to me nearly obscene. (Iris Murdoch)
- (9) There was certainly no evidence that such a letter had ever been written or received. Was it an attempt on De Sousa's part to establish his bona fides -- to make his visit appear natural and even expected? (Agatha Christie)
- (10) ... and here were these two panting and perspiring young women weighed down with heavy rucksacks and without the least idea how to dress themselves so as to appear attractive to the other sex. (Agatha Christie)
- (11) It called for resolution to continue his ascent without undigested pauses, so as to appear merely as leisurely as a captain has a right to be, and yet neither timid nor awkward. (C.S. Forester)
- (12) ... if the disturbance was due to start then and I was brought here as a witness the person might have tangled something so as to appear to be elsewhere at the time. (Dorothy Sayers)
- (13) At the bar she made it appear that she observed the limit; but she had started drinking at home. (Saul Bellow)
- (14) "You have a way of summing up the world's wisdom in a couple of banal sentences. The effect isn't to make you seem wise, but to reduce all the philosophers to the level of twenty-one-year-old girls. (Herman Mouk)
- (15) A blow against his head made it seem to explode; he felt only dimly a jerking at the chain on his cuffs. (C.S. Forester)
- (16) The next days were horrendous, school as I'd never imagined it. It made midshipmen's school, which I resolved to cherish as a special once-in-a-lifetime torture, as the ultimate degradation of the thinking man's mind, seem relaxed. (Paul Mandel)
- (17) We were glad whenever Mr. Barksdale called Father, because it cheered him and made him seem livelier than anything else during these last years. (Peter Taylor)
- (18) ... the upper part of the face, its cheekbones more accurately described as 'large' than 'prominent', making the eyes seem deeper-set than they actually were. (John Main)
- (19) She accepted their invitations calmly, and dined and danced with them placidly and yielded a kiss at the door, after some formal reluctance, in a way that made it seem an old-fashioned courtesy. (Herman Mouk)
- (20) There was an air of forced gaiety about the proceedings. Everyone was determined to appear gay and unconcerned. (Agatha Christie)
- (21) Renauld sees only one escape -- death. He must appear to die, in reality escaping to another country where he will start again under an assumed name... (Agatha Christie)
- (22) If we look at them objectively, we will find that they are civilized by some criteria and uncivilized by others, just as our culture should always appear to us. (Mayne Dyer)
- (23) They can appear to intercept me, kidnap me, arrest me -- anything that seems most convincing. (Helen McInnes)
- (24) ... it was rather dark; waiters appeared soft-footed out of the twilight, bearing silver dishes with the air of patting in some holy rite. (Agatha Christie)
- (25) Palmer appeared, in my generally admiring vision of him, as a complete and successful human being. (Iris Murdoch)
- (26) ... it does not alter the situation whether the fear is recognized as such or appears only as a vague uneasiness. (Karen Horney)
- (27) The answer is that primarily this feeling of intrinsic weakness is not a fact at all; what is felt as weakness and appears as weakness is only the result of an inclination towards weakness. (Karen Horney)
- (28) Automobiles, jeeps, trucks, personal carriers, an occasional tank, some dismembered beyond recognition. A few were unmistakably American, others appeared to be French, and the older battered ones looked as if they might have been Japanese. (Winston Groom)
- (29) ... the Huyen matrons were at their morning coffee now, and even with their mouths full they all appeared capable of talking at once. (Alistair Maclean)
- (30) I mean he didn't paint things as they appear to most people. (Howard Fast)
- (31) When the two girls came out to the bright sunny street, Marsha no longer appeared so transformed. The heavy features of the face were the same after all. Though Marsha had quite started away the pudginess. (Herman Mouk)
- (32) Sir Bartholomew was entertaining a party of friends at his house in Yorkshire. Sir Bartholomew appeared to be in perfect health and spirits, and his demise occurred quite suddenly at the end of the dinner. (Agatha Christie)
- (33) "Everybody appears to be in the expected state of nervous agitation, high excitement, general fatigue, and strong irritation which are characteristic of preparations for this form of entertainment. (Agatha Christie)

- (34) He appeared, instead, to be sunk in some deep and private gloom, from which he roused himself enough to look up almost apathetically as his son closed the door. (Alistair Maclean)
- (35) Mr Elderby wondered what she was doing in Exhampton. She was well dressed in a demure and provocative style, and did not appear a closed relation of the deceased, and still less could be labelled as one of the idle curious. (Agatha Christie)
- (36) "I know. Have you formed any personal opinion of our last recruit yet?" "He's intelligent, tough, strong, and appears to have been without a nervous system. (Alistair Maclean)
- (37) ... was a cheerful, rubicund, genial character who appeared to be in perpetual danger of breaking out into gates of laughter. (Alistair Maclean)
- (38) The count, with a lean aquiline face, and greying hair brushed straight back from his forehead, appeared to be in more than tolerable health. (Alistair Maclean)
- (39) You're quite sure it was Rayburn? Pagett appeared to be perfectly convinced of that. It was the only part of the story he wasn't vague about. (Agatha Christie)
- (40) Example (15) describes a physical contact but it is also an implied purpose, to open the lock, and appears to be a form of understatement for literary effect. (Charles Kint)
- (41) Parker sat...examining his soul. He saw it as a spider web of facts and lies that was not at all important to him but which appeared to be necessary in spite of his opinion. (Flannery O'Connor)
- (42) Something in his own voice, as well as in his bodily sensations, gave Conway a renewed impression that he had been very slightly doped. Mallinson appeared to have been similarly affected, though he seized the present chance to remark:.... (Dorothy Sayers)
- (43) ... but it was a scene of almost unbelievable decay and squalor. Every piece of furniture -- there seemed not many of them -- appeared to be broken, perhaps by the weight of this enormous creature. (Catherine Gaslin)
- (44) He was particularly interested, as a student of affairs, in the way the valley population was governed; it appeared, on examination, to be a rather loose and elastic autocracy, operated from the Tamaseery with a benevolence that was almost casual. (James Hilton)
- (45) The alternative remained open, and the more I reflected the more it appeared to be the only alternative; that Jean Pierre had written a good novel. (Iris Murdoch)
- (46) Before his grace could broach the matter in hand, Cathcart spoke rudely and harshly, appearing to be all on edge, and entreating to be left alone. (Dorothy Sayers)
- (47) Someone was standing up in the next cart destined to receive fire, waving a white handkerchief frantically. Hornblower looked through his glass; the man appeared to be an officer of some sort, in his blue uniform with red epaulettes. (C.S. Forester)
- (48) Half of the houses appeared to be shops -- open on one side, where were displayed for sale a few handbills of fruits, or three or four eggs. (C.S. Forester)
- (49) I am not sure how long I lay upon the floor. Perhaps an hour, perhaps two or three hours. When at last I pulled myself up to a sitting posture it appeared to be afternoon. It was certainly another world and another time. (Iris Murdoch)
- (50) The stream of reminiscences ran remorselessly on... Paggett, supporting himself with one hand against the brickwork of the lodge, appeared lost in the long ago. (Iris Murdoch)
- (51) Harlow didn't seem to care one way or another whether his face was washed; whatever his thoughts were, and in the circumstances it would have taken an idiot not to read them aright, his entire attention appeared to be concentrated on the content of that bottle of Martell, the picture of a man, if ever there was one, who desperately and urgently sought immediate oblivion. (Alistair Maclean)
- (52) The broad highway directly beneath me -- and about seventy feet beneath me -- appeared to be inextricably jammed with clanging tramcars, hooting vehicles and hundreds upon hundreds of motor-scooters and bicycles, all of whose drivers appeared bent on suicide. (Alistair Maclean)
- (53) He sat and stared at the sea, which appeared all surface and twinkle, far shallower than the spirit of man. (Henry James)
- (54) Before I could move a hand, she appeared to leap from the window into space. I rushed and looked out. To my horror I saw her hanging by her hands from the roof... (Agatha Christie)
- (55) The officers on the quarterdeck looked at each other, wondering what the future held in store for them for the commodore to wish them good luck. Hornblower noted the interchange of glances without appearing to see them. (C.S. Forester)
- (56) A figure stood there, pushed back deeply into the recess. Without appearing to have noticed him, Harlow re-entered the café, closed the door behind him and returned to his seat. (Alistair Maclean)
- (57) I was a skinny child, appearing delicate, but actually of robust health and perpetually hungry. (Agatha Christie)
- (58) Joke is one of our oldest friends. I've known him ever since he was a child. He appears to be quite a old person, but he's really a dear -- always the same -- always to be relied on. He's not gay and amusing but he's a tower of strength -- both I and Donald relied on the strength a lot. (Agatha Christie)
- (59) Your arguments appear to be most convincing, but the evidence may bear some other interpretation. (Dorothy Parker)
- (60) While it may appear to the researcher or casual observer that No-Limit people are simply stronger and more independent people, the truth is that they have really decided for themselves that they... (Wayne Dyer)
- (61) They appeared improbable, irresponsible, and wholly fortuitous. In actual fact, of course, they were nothing of the sort. (Agatha Christie)

- (62) The city was really much smaller than it had appeared to be on my first view of it. (Iris Murdoch)
 (63) "This is a child," Barbara told herself. It's not a mask or an image she wears. She is actually what she appears to be. (Howard Fast)
- (64) The Laffey home, from the public road, appeared to be nothing more than a wall of some concrete material, the color livened with a tint of ochre. (Elika Kazan)
 (65) ... and whatever batzi may appear to be, he has a nose like a bloodhound. You're very lucky that he approves of you. It's an open Sesame. (Howard Fast)
 (66) ... a young man... was standing in the roadway. He was wearing khaki slacks and sandals and his torso appeared to be covered by a curious tightfitting garment. As NBP came nearer he saw his mistake. It was not a garment. He was naked to the waist. (Michael Gilbert)
- (67) That type of woman, who appears to be so competent and strong, feels the onset of old age and infirmity more than most, but she cannot say so, cannot give way and confess to human weakness. (Susan Hill)
- (68) He realized that the airport general manager was more astute than he appeared. (Arthur Hailey)
 (69) But Hornblower was not as mad as he appeared. (C.S. Forester)
 (70) Leaving his friend to mind the baby, Costa walked slowly down the ramp to the waterlevel office. He was an unimpressive figure, his left arm in a sling, his middle too heavy, his black hair disordered, his rolling tread pigeon-toed. But he seemed completely self-possessed. (Elika Kazan)
- (71) He seemed pleased and swayed enthusiastically to and fro as if someone were now agitating the strings. (Iris Murdoch)
 (72) She seemed a little recovered and went out to organize the removal men. (Iris Murdoch)
 (73) Her whole face seemed to have grown smaller, and this made her eyes stand out unnaturally. (Herman Wouk)
 (74) You get old, your heart, your liver, your lungs seem to expand in size, and the walls of the body give way outward, swelling, she thought, and you take the shape of a jug, wider and wider towards the top. (Saul Bellow)
 (75) ... I looked into her room and she was asleep, she seemed quite ordinary and peaceful, and so I went back up again... (Iris Murdoch)
- (76) I said so to Mrs Blair -- but she seemed to quite welcome the idea, and remarked that if I were murdered it would be really a terrific scoop for Anne. (Agatha Christie)
 (77) ... the heat seemed trapped even in these lofty rooms, and I tossed and lay wakeful on a sweat-soaked pillow. (Catherine Gaskin)
 (78) ... the bird-like man seemed grateful and responded at once. (John Wain)
 (79) Her face seemed almost gray and there was a look of dull recognition in her eyes... (Flannery O'Connor)
 (80) The engine seemed to miss a beat, and Kibel worked at the controls frantically. (Jack Higgins)
- (81) Indeed she seemed to gain strength rapidly once off the ship, and had drunk a bowl of soup and nibbled a slice of toast to her... (Colleen McCullough)
 (82) It (about blood) seemed to have seeped through the chest, and the valve immediately lifted up the lid of the chest. (Agatha Christie)
- (83) The thing seemed intended, and in that perspective Georgie's action, though hideously upsetting and painful, counted chiefly as a clearing of the decks. (Iris Murdoch)
 (84) But Mary shook her head, huddled close to the fire, staring at the moon. Karanja, too, seemed transfixed by it, and suddenly I realized there was a circle of light round it... (Hammond Innes)
- (85) And yet... "He seemed suddenly to realise that what he was saying could not mean very much to me, and broke off with a laugh. (James Hilton)
 (86) Amos Cabot was not at all like this. He seemed to enjoy seeing himself, and when he lost the election and his face vanished he seemed not perturbed. (John Cheever)
 (87) There seemed nowhere to go... and Geary began to hear the drums more loudly than ever... (John Wain)
- (88) A mercurial girl, she seemed withdrawn and remote that night, and the silence was hardly companionable. (Alistair Maclean)
 (89) But he had a curious sensitivity; he seemed to know what was in people's minds, and when one was past his brusqueness he was a gentle man. (Catherine Gaskin)
- (90) Our bodies seemed to mould together, and then, knowing that the complete fulfillment was denied us, stiffened and hardened and finally suffered the agony of withdrawal. (Iris Murdoch)
- (91) The four seconds of the flight of the shot seemed to stretch out indefinitely, and then at last he saw the jet of water tap up into brief existence, fully two hundred yards to the right. (C.S. Forester)
- (92) He went into technical details. His face was flushed and his eyes shone. The subject seemed to fascinate him, and Mr Satterthwaite noted that he seemed to have a thorough grasp of what he was talking about. (Agatha Christie)
- (93) ... had noticeably lost weight in both body and face, the already trenched lines in the latter seemed to have deepened, and it was possible even to imagine an increase in the silver on that magnificent head of hair. (Alistair Maclean)
- (94) It seemed bigger than a county in Illinois, and as they stepped out into it for the first time, it was fortunate that they could not envisage the incredible obstacles they would face in trying to hold on to it. (James Michener)
- (95) The people there seemed pallid and wilted, defeated by the summer, and they felt hearty and powerful in comparison. (Samuel Shellenbarger)
- (96) The bruise seemed bluer, creeping round the eye socket, and the eye itself was reduced to a watery slit. (Iris Murdoch)
 (97) Jacobson's income, it appeared from them, amounted to at least twenty times as much as he could reasonably expect to earn as a chief mechanic. (Alistair Maclean)

- (98) "From the rapidity with which you reached the rendez-vous, it appears that your passage was even quicker than ours", said Bolden. (C.S. Forester)
- (99) I glanced at Conway and judged that he was enjoying it all, which appeared very natural, in view of his own musical past. (James Hilton)
- (100) From the looks exchanged among the six men...his presence appeared to be giving rise to an increasing degree of veatation. (Alistair Maclean)
- (101) That it appeared to be an entirely innocuous industry was evidenced by the fact that tourists who occasionally happened by were smilingly invited to come inside and look around. (Alistair Maclean)
- (102) Paradoxically, despite having lost his long struggle to maintain control of the hotel, the St. Gregory's proprietor appeared more amicable and relaxed than at any time in recent weeks. He wore a carnation in his button-hole and greeted the visitors cordially, including Mr Dempster whom Peter introduced. (Arthur Hailey)
- (103) The Prefect appeared absolutely thunderstricken. For some minutes he remained speechless and motionless, looking incredulously at my friend with open mouth, and eyes that seemed starting from their sockets, then -- apparently recovering himself in some measure -- he seized a pen... (Edgar Allen Poe)
- (104) Captain Demerest glanced briefly around the coffee-shop and, observing Mel and Tanya, gave a curt, cool nod. Then, checking his watch, and with a final word to the other captain, he strode out. 'He appeared in a hurry,' Tanya said. (Arthur Hailey)
- (105) To transfer this, the animal cages, the prefabricated offices, the fair-ground booths, and Bruno's ramshackle men-talist theatre, not to mention the animals and the circus, members to the coaches and flat-cars, was a massive undertaking that to the layman would have appeared well-nigh impossible: the circus...performed the task with an almost judicious ease. (Alistair Maclean)
- (106) ... your evidence today has made matters very complicated. I would rather suspect the scouts than the S.C.R., I admit, but when these hasty accusations are made by the last person known to have been in the same room with the manuscript, I can only say that -- well, that it appears to me injudicious. (Dorothy Sayers)
- (107) She was a handsome dark-haired woman. Her mouth was hard and her black eyes gave absolutely no clue to her emotions. She appeared perfectly self-possessed. (Agatha Christie)
- (108) "I have never seen that dress to my knowledge before...it appears to be made for a slender person of medium height. (Dorothy Sayers)
- (109) The witness continued: "After searching the room I questioned all four of the people who appeared to be most possibly concerned. (Michael Gilbert)
- (110) A good man often appears gauche simply because he does not take advantage of the myriad mean little chances of making himself look stylish. (Iris Murdoch)
- (111) She went up the dimly lit stairs, full of an indefinable fear. She seemed to be seeing the torn blue jinnelium on the steps, and smelling the incense odors from the ground-floor apartment, for the first time. (Herman Wouk)
- (112) She seemed to be laughing, but he wasn't sure. (Dorothy Sayers)
- (113) He seemed to be trembling, perhaps from the recent attentions of the wind, perhaps from nerves and alcohol. (Iris Murdoch)
- (114) Lady Stubbs was reclining in a big arm-chair a little distance from the others. She seemed to be paying no attention to what was going on round her. (Agatha Christie)
- (115) I watched the barmaid, and I listened to the music which seemed to be causing the pavement to shake. (James Baldwin)
- (116) Today the choice of justice seemed to be causing her the most uncertainty and she vacillated -- through an exchange with the unseen order taker lasting several minutes over the comparative merits of pineapple, grapefruit and orange. (Arthur Hailey)
- (117) The famous oil-fired central heating seemed to be making little impression on the temperature in the room. (Iris Murdoch)
- (118) The conversation seemed to Wormold to be reeling out of sight. (Graham Greene)
- (119) The rain and wind-battered earth seemed to be settling once more into its fertile sleep. (Catherine Gaspin)
- (120) "That's just it. With one half of her mind -- no, more than half, two thirds at least -- she really doesn't seem to think of anything but him. And with the rest she seems to be drifting." (Herman Wouk)
- (121) He had the unvarying habit, whenever the taxi seemed to be careening toward a collision of dropping to the floor of the cab and twisting the wheel sharply, then bobbing up to see where he was, and how he was doing. (Herman Wouk)
- (122) The seat seemed to be gyrating like a Coney Island ride. (Herman Wouk)
- (123) "Regulations," Schaefer replied, "Where you go, I follow. Which seems to be stretching bureaucracy to the limits." (Jack Higgins)
- (124) He did not speak outright to him, for Mr Murphy lived in a state of indirection, and often when he used the pronoun "I", he seemed to be speaking of someone standing a little to the left of him. (Jean Stafford)
- (125) On the porch, Ethel and Teddy waited while an intense whispered consultation went on. Costa seemed to be making a point about Ethel. (Elia Kazan)
- (126) He stood there a moment swaying on his feet and looking glastly. He seemed to be listening; then with an effort he seemed to pull himself together and, opening the door into the tower room, called out something about there being no harm done. (Agatha Christie)

- (127) ... that I had accepted with such completeness the picture of things which Palmer and Antonia seemed to be offering. (Iris Murdoch)
- (128) Her eyes, rather like those of an intelligent spaniel, seemed to be trying to say something. (Agatha Christie)
- (129) Under the strain of fatigue and headache, and the emotional tension of the weekend, Marjorie's sense of reality was giving way. Things seemed to be happening about her in a brightly colored noisy dream. (Herman Wouk)
- (130) He seems to be thriving. In fact, the cook tells me he's eating ten men. (Herman Wouk)
- (131) The old man's expression had not changed. He seemed to be waiting for something that had not yet happened. (Elita Kazan)
- (132) What was he like, the doctor, I mean. Did he seem to be looking forward to the house-party? (Agatha Christie)
- (133) ... stopped reading and seemed to be only admiring the balustrade's boxes of trailing geraniums and sprightly verbenas. (Helen MacInnes)
- (134) ... they are outraged at how sex seems to be infiltrating television, advertising, films, books and everything else. (Wayne Dyer)
- (135) I had never appreciated that those damned barges moved so slowly. As far as the coastline of Huyes was concerned, the barge seemed to be standing still in the water. (Alistair MacLean)
- (136) ... and he seemed to be enjoying his role of chief not least, I guessed, because he had had Maria Stuart as his assistant. (Alistair MacLean)
- (137) Hercule Poirot's voice was dreamy. He seemed to be addressing himself to space rather than to his listeners. (Agatha Christie)
- (138) She even seemed to be walking in a less girlish way. (Alex Haley)
- (139) He seemed to be driving more and more slowly, and the rain and the mist became more impenetrable. (Agatha Christie)
- (140) He felt the boat swing round. The current seemed to be carrying her over to one side, and he was content to let her go. (C.S. Forester)
- (141) If you are stopped by a policeman for what you consider no reason at all, you may appear to be quaking and shivering on the outside, but inside, knowing you have done nothing wrong, you may have all the security in the world. (Wayne Dyer)
- (142) Costa, still seated, his face still warmed by the sun, didn't appear to be watching them. (Elita Kazan)
- (143) The pair passed at twenty feet, and being properly mothered young men, they didn't stare at the topless young woman. At a decent distance they sat and appeared to be killing time. (Elita Kazan)
- (145) The young fellow appeared to be chanting some sort of repetitive litany. (Iris Murdoch)
- (146) Parker's heart began to beat faster and faster until it appeared to be roaring inside like a great generator. (Flannery O'Connor)
- (147) Harlow seemed unaware of the presence of them, for he looked neither to left nor to right, but it was a safe assumption that these eyes missed nothing, an assumption borne out by the fact that, apparently without noticing them, he veered direction towards where Mac Alpine and Dunnet sat. (Alistair MacLean)
- (148) He was shivering violently in the cold night air but seemed to be entirely unaware of this. (Iris Murdoch)
- (149) The temperature in that icily cold metallic tomb had fallen to many degrees below freezing point, but everyone appeared to be completely unaware of the fact: the classic example of mind over matter, and heaven knew that minds could rarely have been more exclusively and more oppressively possessed than those of the men in those weird and alien surroundings. (Alistair MacLean)
- (150) ... and then the warm breathless rush of the drug seemed to come over me again. (John Wain)
- (151) He looked across the room at Beatrice Severn, and she seemed to him to belong to the same world as the fatal drink, the hopeless journey from Ireland, the surrender in the forest. (Graham Greene)
- (152) Her 'no place' seemed to search the universe and fold it into a box. (Iris Murdoch)
- (153) All the best things about the city seemed somehow to have an echo and reflection in the tall, straight, dependable girl. (Irwin Shaw)
- (154) The trees, fields and hedges, no less than the animals, seemed to resent the intrusion of light into its December pattern... (John Wain)
- (155) If there had been a conversation in progress when he arrived, his presence seemed to have wilted it effectively. (John Wain)
- (156) His veins seemed to empty of blood and fill with a hard, flinty dust, which shifted about like powder as he walked, and, when he lay down at night, poured sluggishly into his head. (John Wain)
- (157) ... his voice lowered and his words seemed to pull apart from each other. (Paul Mandel)
- (158) Once Harry Rayburn's face seemed to come to me out of the mist. (Agatha Christie)
- (159) He felt no floor under his bare feet; he seemed to walk beneath the palpable weight of the grim turning faces. (William Faulkner)
- (160) A veil of guilt seemed to divide me from her, and with it a sense that the blow of discovery had at least crippled, if not killed, my love for her. (Iris Murdoch)
- (161) He pulled at the remaining ligature, but all he got was a cry of pain from Bush -- which seemed to go clean through Hornblower's heart. (C.S. Forester)

- (162) By night it had the look of theatrical gloom -- a compact stage-set, dimly lighted, for even here the heavy walls seemed to eat the brightness of the lamp set high on the surrounding houses. (Helen MacInnes)
- (163) Potrot looked at her curiously. She seemed, he thought, to have aged about ten years since the time early in the afternoon... (Agatha Christie)
- (164) The shadowy outline of the towering crane seemed to reach up almost to the clouds, the tip of its massive horizontal boom lost in the surrounding darkness. (Paul Mandel)
- (165) I took the cigarette and lit it and then all the pain and fatigue took me by the forehead and seemed to squeeze me into rags. (Paul Mandel)
- (166) Paddy's heart seemed to block his throat. (Colleen McCullough)
- (167) I stared fascinated at that tired strained face, a face with the look of hunger upon it and eyes that seemed to consume him with their own fire. (Catherine Gaslin)
- (168) The place was airless and smelt vilely of alcohol and stale tobacco which seemed to linger visibly in the air as I pulled the curtains apart. (Iris Murdoch)
- (169) And always, about the vines, the humming-birds danced on the air with the myriad stirring of their wings, seeming to stand still before one's eyes, so fast were the movements of their wings. (Catherine Gaslin)
- (170) I saw Palmer and Honor naked in each other's arms, enlaced, closer and closer, until they seemed to have become one person. (Iris Murdoch)
- (171) Silence, the night seemed to have swallowed up all activity. (John Main)
- (172) Her utterance surprised them all, herself as much as anyone. It was pure impulse. The words seemed to jump out of her throat. (John Main)
- (173) Efficiency seemed to radiate from the man. (Agatha Christie)
- (174) The great room, which was a Cotswold farm whose scored crevices the warm oily air, gently circulating, seemed to sift down an ancient dust. (Iris Murdoch)
- (175) (About a face) It seemed to sway and hover above the jostling mass of people, disembodied almost. (Catherine Gaslin)
- (176) The cicadas sang, the whole earth seemed to hum of its fertility. (Catherine Gaslin)
- (177) The whole world seems to have gone completely mad about me. (James Hilton)
- (178) Georgie shook her head and made a gesture with her hands which seemed to set Antonia's remark aside as being unintentionally inappropriate. (Iris Murdoch)
- (179) Her small twinkly eyes seemed to say, "You expect me to be the buffoon?... (Agatha Christie)"
- (180) It was a wonderfully warm intimate smile; it seemed to say that Noel Atman and the person he was smiling at were equals, rather better than most people, and sharers of some secret knowledge that was at once ironically amusing and a little melancholy. (Herman Mouk)
- (181) Anyhow, Edwin seemed to think that you might perhaps know something -- how shall I put it -- something useful about that young Desmond Leeworthy. (Agatha Christie)
- (182) ... whose features seemed to indicate a strong dash of negro blood... (C.S. Forester)
- (183) Far ahead, where coast-line, sea, and sky formed their mighty angle, there were little dots which seemed to indicate a city on the shore. (Stephen Crane)
- (184) Eventually, as the visitors showed no signs of departing, and gay laughter seemed to indicate things were indeed going on nicely, I knocked on the door and after a suitable interval went in. (Iris Murdoch)
- (185) Yet there was a serenity in her expression which seemed to show that at the moment at least she was not worrying about being an old maid. (C.S. Forester)
- (186) Airline delays seemed to arouse a latent savagery in those affected by them. Men talked insultingly to women telephonists, and even people who at other times were courteous and mild-mannered turned snarly and disagreeable. (Arthur Hailey)
- (187) Scodd seemed to have fallen into a trance: his eyes were groping in Charles's direction as if searching for an invisible opponent. (John Main)
- (188) Captain Segura squeezed out a smile. It seemed to come from the wrong place like toothpaste when the tube splits. (Graham Greene)
- (189) The majesty, the familiarity, of these buildings seemed to add solemnity to my rite, as when old patriarchs come to grace a marriage. (Iris Murdoch)
- (190) Only it seemed to have become extremely difficult to speak, as if I were being subjected to some paralyzing ray. (Iris Murdoch)
- (191) Then all at once she seemed to explode like a piece of machinery that had been given one ounce of pressure too much. (Flannery O'Connor)
- (192) El Supremo stepped on the quarterdeck with a keen glance round him, and then, while Hornblower looked at him, the interest faded from his face and he lapsed into the condition of abstracted indifference in which Hornblower had seen him before. He seemed to listen, but he looked over the heads of Bush and Gerard and the others as Hornblower peered at them. (C.S. Forester)
- (193) All I knew then was that the pressure of Fergus's hand on mine seemed to increase, as if he would will some strength back into me. (Catherine Gaslin)
- (194) ... some of the gusts rose to shrieks and seemed to claw at the house, as if it would pull its bones apart. (Catherine Gaslin)
- (195) Instead, he learned suddenly and unobtrusively on the handle and appeared to pivot in some way on his forearm. (John Main)

- (196) 'I want to see Inspector Narracott,' said Emily, 'is he here?' Agin there was a pause. Mr Duke seemed uncertain how to answer. At last he appeared to make up his mind. He smiled -- a rather curious smile. (Agatha Christie)
- (197) He welcomed Barbara with youthful enthusiasm. "I must say that you appear to have survived it (arrest by Gestapo) very well indeed." (Howard Fast)
- (198) I waited for a time that seemed polite, but Andrew Maxwell appeared to have nothing more to say to me. (Catherine Gaskin)
- (199) "And here's the list of missing persons... Next one, Professor Hargraves. Did not turn up and sent no note of excuse." Harcastle did not appear to consider Professor Hargraves. (Alistair Maclean)
- (200) From what he could see from a cursory glance there was no book here of any real interest or, indeed, any book which appeared to have been read. (Agatha Christie)
- (201) "You don't appear to have brought anyone with you." (Private Schulz - a series on Swedish TV)
- (202) "This has been the quickest passage I have made to these latitudes," said Osborn. "I begin to regret it now that it appears to have prevented our seeing any of you." (Isak Dinesen)
- (203) Just at the moment when it seemed their silence was assuming the form of contest to see who could hold out longest. (John Mair)
- (204) It seems that the girl is actually stopping there with a cousin or something. (Agatha Christie)
- (205) ... but it seems he's been using Madame Rose as post office. (Jack Higgins)
- (206) It seemed to Marjorie that she was discovering New York. (Herman Mouk)
- (207) As she watched her daughter burst into bloom on Central Park West her own miserable adolescence came back to her, and by contrast it seemed to her that Marjorie was living the life of a fairy-tale princess. (Herman Mouk)
- (208) But I did not in general feel envy of Arnold Baffin for one very simple reason: it seemed to me that he achieved success at the expense of merit. (Iris Murdoch)
- (209) But the squealing of the swine, it seemed to Kunta, was no uglier than the language of the toubou who so closely resembled them. (Alex Haley)
- (210) It seemed to me that in the hour of success I had promised not only my mother that I would shun -- Sally Rand -- the promise was to all those who had wanted to go where we were going and couldn't. (R.V. Cassil)
- (211) It seems that instantly the beach was populated with men with blankets, clothes and flasks and women with coffee-pots and all the remedies sacred to their minds. (Stephen Crane)
- (212) When the last bell rang, the last class ended, I let out my breath. It seemed I'd been holding it for all that time. (James Baldwin)
- (213) They steamed past the Empire State Building and it seemed to Marjorie that she was in the building watching the thing go by. Instead of the other way around. Never in her life had she felt so totally dislocated, so outside reality. (Herman Mouk)
- (214) Marjorie's gesture at last was not even a nod; it was a slight, very slight, ashamed dip of the head. It didn't seem to her she willed the movement; it happened. (Herman Mouk)
- (215) A train whistled as it passed through the valley. It seemed to me that I had heard Nina's voice. (Yehuda Amichai)
- (216) She lay back on the rock and watched the sky. It seemed to her that she could walk into the sky, stepping through clouds endlessly. (Lestine Shtiko)
- (217) Born, I am told, with the umbilical cord around my neck, it seems to me I will always come close to strangulation trying to deliver myself from the past into my future. (Philip Roth)
- (218) The waves were no longer waves, just moving masses of water, capriciously tending, it seemed, in any and all directions, but in the main still bearing mainly easterly. (Alistair Maclean)
- (219) What there was now was my relationship with Julian, ... urgent and puzzling and historically dynamic, changing, it seemed, even from second to second. (Iris Murdoch)
- (220) Like most of the rest, she simply nodded -- but with the trace of a smile, it seemed to Kunta. (Alex Haley)
- (221) ... he commented on the Lavette Strpyard. "It would appear," he said, "that here we have one of the largest and most impressive potentials on the West Coast. (Howard Fast)
- (222) He assured me there's nothing further legally that the sponsors can do. It would appear that the case is virtually defunct. (Arthur Hailey)
- (223) It appeared that my charming little girl had not exactly shown up with the United States cavalry. (Paul Mandel)
- (224) This time it appeared that the elevation was correct, for the shot pitched well up to the boat... (C.S. Forester)
- (225) "Don't mind Leo, Reggie, he's mislaid his manners this morning." Mr Pomfret appeared to think that somebody else's manners had been mislaid, for he replied... (Dorothy Sayers)
- (226) His story was extraordinarily vague and rambling and it was a long time before I could make head or tail of it. To begin with, it appears he caught sight of a man behaving suspiciously. Those are Pagett's words. (Agatha Christie)
- (227) "You've just given your brother Seldon Bark. He's understandably delighted." Barbara shrugged. "He appears to have wanted it very much." (Howard Fast)
- (228) ... and the heat pouring up from the hatch made all the forward part of the ship appear to dance and shimmer in the sunshine. (C.S. Forester)
- (229) ... young men who wanted to talk about detective fiction. They appeared to have read a good deal of this kind of literature... (Ngũgĩ Marsh)
- (230) The number I wanted was 61, and could I find it? No, I could

- not. Having studiously followed the numbers from 1 to 35, Wilbraham Crescent appeared to end. A thoroughfare barred my way. (Agatha Christie)
- (231) She made off across the lounge and addressed the young agitator who started, shook his head, appeared to apologize, gave a hurried look at his watch, and darted out by the entrance. (Dorothy Sayers)
- (232) He just stood stiff in the center of the rug, in his hat, the shaggy iron gray brows twitching slightly above the pebble-colored eyes as he appeared to examine the house with brief deliberation. (Howard Fast)
- (233) He had been... hard at work upon cases, some of which trickled through into newspaper columns, while others appeared to settle themselves in discreet obscurity. (Dorothy Sayers)
- (234) Merchant advanced upon Richard with soft extended hand... Richard appeared to permit, rather than return, a long pressure of his hand. (Dorothy Sayers)
- (235) She found herself arguing passionately with him about the prospects of the Ottawa conference, of which he appeared to entertain no great hopes. (Dorothy Sayers)
- (236) I could see neither of the musicians: the sound appeared to come from the far side of the haystack the matrons had just finished building. (Alistair Maclean)
- (237) Mr. Pettigrew-Robinson appeared to give evidence with extreme reluctance. (Dorothy Sayers)
- (238) "He would appear to have a religious revival on our hands," Van Buren observed. (Jack Higgins)
- (239) Parker sped on, then stopped. His heart too appeared to cut off; there was absolute silence. (Flannery O'Connor)
- (240) Trains passing appeared to emerge from a tunnel of trees, hit for a second by the cold sky, vanish terrified into the woods again. (Flannery O'Connor)
- (241) The old man didn't appear to have heard. As Petros approached closer, he dropped his head. (Elia Kazan)
- (242) "When you can stay on deck, m'am, since you wish it, unless this gale increases -- and I fancy it will," "Thank you, Captain," she replied. There was a look in her eyes that seemed to indicate that the question, as to what would happen if the gale increased, was not as decided as the captain appeared to think. -- (C.S. Forester)
- (243) Of this contention not a jot of little, not the slightest shadow of evidence had been put forward except, indeed, that of the extraordinary witness, Robinson, who appears to bear a grudge against his whole acquaintance, and to have magnified some trifling allusion into a matter of vast importance. (Dorothy Sayers)
- (244) In his surprise he spoke English, and seeing that this appeared to have gone over his visitor's head, he repeated it in colloquial French. (Michael Gilbert)
- (245) In the absence of the distraction of the rapids they were conscious of the wind again now, and Homblower turned the boat's bows into it and pulled strongly at the sculls. Past experience appeared to have demonstrated conclusively that this was the best way to allow the current a free hand to take the boat down-stream and away from pursuit. (C.S. Forester)
- (246) At any rate, we all thought he was still in reduced commission -- some sort of a school or depot ship. However, it appears he was not in reduced commission the night of HK-2. (Paul Mandel)
- (247) From there, Harlow moved to the last of his four objectives -- Jacobson's room. Jacobson, it appeared, was either less discreet or less sophisticated than either Traccia or Neubauer. He had two bank-books and when Harlow opened them he sat quite still. (Alistair Maclean)
- (248) I drove out to the house -- it was about half past nine when I got there -- and asked for her. It appears she left home this morning -- went up to London for the day -- that's what she said. (Agatha Christie)
- (249) It appears that two masked men entered the bedroom, gagged and bound her, whilst forcibly abducting her husband. This we know at second hand from the servants. (Agatha Christie)
- (250) Towards late afternoon, when Barnard and Mallinson, who had been arguing, appealed to him on some point, it appeared that he had fallen asleep. (James Hilton)
- (251) ... I could see the drops of moisture upon her short black hair which seemed like a bedraggled cap set with small gems. (Iris Murdoch)
- (252) I walked on under Waterloo Bridge and saw through the tilting, slightly lifting mist the long gracious pillared facade of Somerset House, receding, swaying, variously browned and grayed, it seemed like a piece of stage scenery. (Iris Murdoch)
- (253) I took the sheep's skull... It had been smoothed and caressed and polished until it seemed more like a work of art, some exquisite fabrication in ivory, than one of nature's remnants. (Iris Murdoch)
- (254) ... and as she lay there completely limp with her mouth open, the lifeless air and the deep regular breathing made her seem like waxwork. (Iris Murdoch)
- (255) He played Dalí, made his bed, took sailing lessons, and got his life-saving certificate, but this seemed more like a careful imposture than any sort of participation. (John Cheever)
- (256) Doris would change a travelling salesman whatever she could get but she did it with the regulars for nothing. This seemed less like tolerance than hapless indifference, the absence of vision, moral stamina, the splendid ambitiousness of romantic love. (John Cheever)
- (257) Once, in the gray light, when he felt calm for a whole hour, he had a thought which seemed like a blank wall... (William Golding)
- (258) They always seemed to Michael like exquisitely mannered visitors from the nineteenth century, polite, detached, but

- secretly disapproving of the time and the country in which they found themselves. (Stephen Crane)
- (259) "the boats." I shouted, and what seemed like an endless line of them came out of the harbor. (Paul Mandel)
- (260) He only stopped giggling when he realized with a shock that it was hardly more than two hours back that he had first uttered the prayer about God helping sailors on a night like this. It seemed like a fortnight ago at least that he had last breathed the stuffiness of the inside of that coach. (C.S. Forester)
- (261) ... and when he was a boy the pleasure steamers on the river at home seemed to him like liners. (William Golding)
- (262) She had a slight fever for a couple of days, but it didn't seem like anything and we just kept her in bed. (James Baldwin)
- (263) They may get angry at the kids for not wanting to visit them, even though a visit may seem to the kids like spending several weeks in a camp with a pushy encyclopedia salesman. (Mayne Dyer)
- (264) ... but now it seemed certain, so solidly based was the rumour, that the Lydia was on her way home. To the scatter-brained crew it seemed as if England was just over the horizon. (C.S. Forester)
- (265) When the third fight ended with a blow delivered so hard the defeated man seemed as though he had been lifted with a kick... (Winston Groom)
- (266) ... and her eight-month pregnancy made her seem as if she had swallowed a pumpkin. (Alex Haley)
- (267) Tom tensed, but he managed to seem as if he totally ignored Asford's remark... (Alex Haley)
- (268) It seemed as if the inhabitants of the Khan had divined in some mysterious way that the altercation touched them. (E.M. Forster)
- (269) It appeared as if the ships were going to collide. (C.S. Forester)
- (270) Meanwhile the lighthouse had been growing slowly larger. It had now almost assumed color and appeared like a little gray shadow on the sky. (Stephen Crane)
- (271) Hornblower was painfully aware that no very great error in his navigation would have brought them anything up to two hundred miles from where he thought he was, and he realised that on a coast like this, littered with volcanoes, one section would appear very like another. (C.S. Forester)
- (272) She too seemed for a moment to want to stay. (Iris Murdoch)
- (273) ... For a dizzy instant it seemed likely that he would release the ladder and slide down helplessly. (John Main)
- (274) He shook the table so that the plates rattled, and for a moment it seemed that he would push it over. (Carson McCullough)
- (275) "Mother!" cried the lad, stupefied. For a moment she seemed to waver, to falter before the anguish in his voice. (Colleen McCullough)
- (276) For a moment it seemed as though the detective was going to break out angrily, but with an effort he controlled himself. (Agatha Christie)
- (277) For a moment it seemed as if he might say something more, and then thought better of it and went out. (Jack Higgins)
- (278) After a moment, during which he seemed to wait for Antonia to speak, he said, "I'm afraid we can't just leave this thing. (Iris Murdoch)
- (279) For Keith Bakersfeld, it seemed for a while as if life would go on as usual. (Arthur Hailey)
- (280) For a moment it appeared to be a toss up as to whether Lord Nasby had apoplexy or not. In the end he swallowed twice and got over it. (Agatha Christie)
- (281) And they never seem to realize that their own rigid thinking is the source of their misery. (Mayne Dyer)
- (282) More to the point, he thought bitterly, he never seemed able to learn from past experience. (Arthur Hailey)
- (283) He was lean and angular, with a hollow-cheeked mournful face, and always looked as if he needed a good meal. Stewards heaped food upon him, but it never seemed to make any difference. (Arthur Hailey)
- (284) The more Kurta thought about it, the only thing that he had found lacking with Noah, in fact, was that he had never seemed to show the slightest personal interest in Kizzy. (Alex Haley)
- (285) He and Matilda had never seemed really close -- but when she died, somehow his own warmth died with her. (Alex Haley)
- (286) Kurta's eyes never seemed to have been closed before one of the Kintango's assistants was shouting them awake for some training session. (Alex Haley)
- (287) The landlord and his two daughters were working hard filling glasses and taking money, but he never seemed to get any closer to being served. (John Main)
- (288) She had an extraordinary knack of never seeming very much surprised, yet of always seeming slightly indignant... (James Hilton)
- (289) This seance business never seems very practical, does it? If the old boy could get through and say he was dead, why couldn't he say who murdered him? It ought to be all so simple. (Agatha Christie)
- (290) Lady Stubbs was a dumb beauty wrapped up in her own self and her own appearance. She never seems to have ordered meals or taken an interest in household management. (Agatha Christie)
- (291) The billows that came at this time were more formidable. They seemed always just about to break and roll over the little boat in a turmoil of foam. (Stephen Crane)
- (292) It always seemed strange to Normoid that he continued to exist for others when he was not there. (Graham Greene)
- (293) The more heated she became, it always seemed, the more desirable she grew. (Arthur Hailey)
- (294) I thought of myself as lost, sunk without trace in a love which now seemed tinged with insanity, and deprived al-

- together of hope. (Iris Murdoch)
- (295) Cohen worried about the effect of the bird's departure on Maurie's schooling but decided to take the chance, first, because the boy now seemed to have the knack of studying... (Bernard Malamud)
- (296) But you will grow up, you will change, more than may now seem to you possible. (Iris Murdoch)
- (297) Inevitable now Honor seemed to be, vast across my way as the horizon itself... (Iris Murdoch)
- (298) ... nor could the old love, poor pathetic thing as it now seemed, heal me of the new. (Iris Murdoch)
- (299) The field now seemed alive with people. (Isak Dinesen)
- (300) That I had so readily jumped at a way of enduring, as it were acting my way through the situation, had, it now seemed, merely postponed the moment of more radical and more dreadful estimate of what had happened. (Iris Murdoch)
- (301) It now seemed absurd to imagine that being herself, she could have reacted otherwise, less humanely, with less sheer sense and kindness. (Iris Murdoch)
- (302) The kitchen, so rashly deserted, now seemed a haven of blessed unconsciousness. (John Main)
- (303) People began to materialize very rapidly at the barrier, and I concentrated on my, as it now seemed impossible task of recognizing the person I was to meet. (Iris Murdoch)
- (304) ... where Arthur and Phyllis had played and fought and sprouted out of these clothes more happily than she now seemed to remember. (Monica Dickens)
- (305) Now she seemed to make her mind up definitely. (Katherine Mansfield)
- (306) I walked on and on, those dusty came breaks seeming now as familiar to me as the hills I had walked behind Sittikh.
- (Catherine Gaskin)
- (307) Yet Denis himself seems now almost invisible. (Iris Murdoch)
- (308) ... a strongly sweetish tobacco smell that seemed now to cling even to my own clothes. (Catherine Gaskin)
- (309) It seems to me now that you do not really want your wife back at all. (Iris Murdoch)
- (310) My own former instinct, which Georgie had endorsed, that an intimate talk about her with Antonia was something that an avoided, seemed now to have been a sound one. (Iris Murdoch)
- (311) They seemed now in spirit to be always waiting. (Iris Murdoch)
- (312) She had shaken the water from her hair which seemed now to be a furry hat, and in her high black skating boots she looked to me like a Gossack. (Iris Murdoch)
- (313) The earth seemed dry and cracked. (Catherine Gaskin)
- (314) We found nothing of interest, however. All seemed square and above-board. (Agatha Christie)
- (315) A couple of instructors buckled me into the gun -- it immediately seemed too big and thoroughly unmovable. (Herman Mouk)
- (316) He wondered if, forty years ago, he himself had seemed as youthful and vigorous as the crew-cut, athletically built young man who stood, uncertainly, before him now. (Arthur Hailey)
- (317) It was a pleasant homely atmosphere. Anne seemed self-possessed and at her ease. (Agatha Christie)
- (318) Hugo Cornworthy was a good-looking young fellow, very correctly dressed. He seemed intelligent and efficient. (Agatha Christie)
- (319) ... her manner seemed to him nervy and a trifle defiant, but she showed real affection for her grandmother. (Agatha Christie)
- (320) That way Marsha might achieve a certain theatrical attractiveness, instead of seeming overblown and frowzy. (Herman Mouk)
- (321) The sergeant was polite and solicitous but seemed uncomfortable and nervous. (Winston Groom)
- (322) Her connexion with Palmer and Honor, since she did not share in the knowledge that was crucial, seemed flimsy and abstract compared with mine. (Iris Murdoch)
- (323) The house at Hereford Square still seemed grey and derelict; after having been half slaughtered it had not yet come back to life. (Iris Murdoch)
- (324) It seemed clear and logical enough that the person who had committed the crimes must have been a person who had been present on both occasions. (Agatha Christie)
- (325) With half my mind still upon it, I must have seemed raw and ill-at-ease. (Catherine Gaskin)
- (326) He noticed that she seemed older, and strained, but the change did not interest him much. (William Golding)
- (327) ... nor can I guess at any motive. It seems absurd -- fantastic. (Agatha Christie)
- (328) He seemed inert, without resolve. (Elia Kazan)
- (329) Samson-Aaron had seemed merely the fun-maker, the heckler of the feast. Now Marjorie saw that he had been nothing less than the soul of it... (Herman Mouk)
- (330) ... but the smaller tarpaulin-covered shape seemed loose, ill-secured, as if the workmen had been in a hurry and had dumped the last load in a bundle. (Helen McInnes)
- (331) Costa nodded slowly. He seemed lethargic, heavy in the head, even stumped. (Elia Kazan)
- (332) The ship seemed suddenly very empty as they moved down the ramp, vanishing one by one up the road into the darkness of the palms. (Hammond Innes)
- (333) He seemed puzzled and uneasy as I showed him to the door. (Hammond Innes)
- (334) When she was hidden from the waist down her breasts seemed unusually pronounced as they jutted above the floor level. (Herman Mouk)
- (335) Chief Moore snook his big head, seemed lost in thought as he stared at the nude body. (James Michener)
- (336) The serenity seemed a paradox as he thought about his cousin's problems. (Philip Roth)
- (337) The mockery had left her face and as she stared at me the griminess of her expression seemed more and more weighted

- with melancholy. (Helen McInnes)
- (338) I had displayed too much emotion. Now instead I must stare with cold curiosity. As I practiced staring at her charged and glowing image it seemed to be dissolving and changing before my eyes. (Iris Murdoch)
- (339) I looked at the telephone box; and as I looked it seemed to take on a strange sudden glory, such as is said to invest the meanest object in the eyes of those who claim to experience the proof of the existence of God. (Iris Murdoch)
- (340) As she spoke that weak thing within her seemed to uncoil, to grow suddenly tremendously strong. (Katherine Mansfield)
- (341) The heavy-set cates was seeming to ignore them as he moved about serving his few white customers. (Alex Haley)
- (342) I could explain nothing and felt that I was creating some entirely false impression. Also I knew that I was not only seeming but also feeling appallingly guilty. (Iris Murdoch)

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CAN *ANY* EXPRESS ANY QUANTITY?

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Any has been aptly described as a "many-splendoured thing". Not the least of its splendours can be attributed to the fact that *any* is a quantifier. This view is accepted by most scholars but it raises a question which is not always easy to answer: if *any* is a quantifier, what quantity does it express? The problem here is not that it expresses no quantity, but rather that it expresses a wide variety of quantities, so wide in fact that it is difficult to reconcile this polysemy with the idea that *any* is a single word insofar as the expression of quantity is concerned. Since our main concern here will be to show the underlying unity of *any*, we shall focus our attention on its quantitative facets. However, such is the diversity of this "extraordinary word" that we shall not be able, in the time at our disposal, to deal adequately with all these facets, let alone its other splendours.

We can best begin by examining some clear cases where the quantity expressed is not in doubt. For example, if I want to find the city hall, someone may well tell me:

- (1) *Ask any man you meet.*

Here the speaker obviously intends me to pick out one man at random from among those I meet and ask directions. Similarly, when showing someone a card trick, one might say:

- (2) *Take any card.*

Again it seems obvious that the speaker intends the other person to choose a single card out of the deck. The meaning of *any* in these two cases can be characterized as 'one'

This first sense of *any* can be contrasted with the sense it suggests in the following sentence:

- (3) *Give me any letters you find.*

Here the speaker seems to have in mind something like 'all the letters, however many this may be'. Similarly, in:

- (4) *Any changes could be made in proof.*

the sense is something like 'all changes that may be required'.

Examples (3) and (4) thus indicate that *any* has the possibility of expressing an all-inclusive quantity, a totality.

So far we have seen that *any* can express quantities as different as 'all' and 'one'. A third sense can be discerned in the following sentence:¹

- (5) *The closest scrutiny is owed to the Anglo-Saxon kennings and the Homeric epithets; if any words and phrases are formulaic, they will be.*

Here the suggestion is something like 'if there really are certain words and phrases which are formulaic...'. Quantity-wise, *any* here appears to have the sense 'a certain number of', that is, some quantity intermediate between 'one' and 'all'. The following use of *any* can be understood in the same way:

- (6) *That any people retain doubts on these questions is symptomatic of two unfortunate conditions of modern existence.*

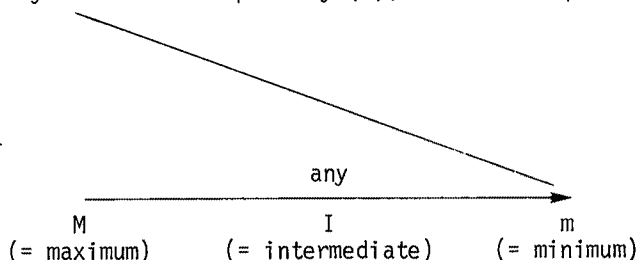
The writer has in mind something like 'that there should be a certain number of persons who...'. Again one gets the impression of a quantity which is neither 'one person' nor 'all people' but somewhere in between the two.

These examples will suffice to show that *any* can express not just one quantity but a range of quantities. An observation of this sort is by no means original. *Webster's Third*, for example, has the following entry for *any*: "one, some or all indiscriminately of whatever quantity". To my knowledge, however, nobody has yet come to grips with the problem of polysemy posed by these data. If *any* is the same word in each of our examples, and I believe most speakers of English would hesitate to declare it otherwise, then underlying these different senses there must be some principle which ensures the notional unity of the word, some meaning potential which can give rise to the expression of quantities ranging from 'all' to 'one'.

The principle to be proposed here is drawn from Guillaume's theory, the Psychomechanics of Language, where it is postulated that language is operative in nature. This postulate suggests that the underlying meaning of *any* (or of any other morpheme with a number of observable senses) is not something static - a particular quantity, for example - but rather something kinetic: some mental operation making it possible for the speaker to obtain a representation of the various senses expressed by the

¹ This example, like most of the others in this paper, has been taken from Sahlin 1979.

morpheme. In other words, if we postulate that the meaning potential of *any* is a movement from a point representing 'all', a maximum quantity, to a point representing 'one', a minimum quantity, then the quantitatively different senses of *any* in discourse can all be seen as resulting from intercepting this movement at the point corresponding to the quantity to be expressed in the sentence being constructed. The following diagram will express this contractive movement from maximum quantity (M), through any intermediate quantity (I), to minimum quantity (m):²



If, then, the potential meaning of *any* is a movement with this contractive form, going from greater to less, one can obtain the actual or contextual meaning of 'all' by holding up the movement at its very beginning (M). The movement will produce the representation of a minimum quantity, 'one', if intercepted at its very last instant (m). The representation of an intermediate quantity can be obtained through an interception somewhere between the first and last instants. The significant point for us here is that this manner of conceiving the potential meaning of *any* - as a movement between limits - permits us to maintain the notional unity of the word and yet account for its variability in usage.³

A second point of theory can be introduced by examining several examples where *any* occurs as a pronoun:

- (7) *If any among the hardy hundreds who sat in the downpour are in doubt about how it comes out, let them take comfort.*

² The reason for postulating a contractive movement (M - m) rather than an expansive movement (m - M) is, ultimately, the hypothesizing character of *any*. This characteristic can be demonstrated only through a comparison with *some*, a discussion which cannot be undertaken here due to limitations of space.

³ For the application of this operational model in the analysis of another problem of English grammar, see Hirtle 1982.

In (7), *any* expresses an intermediate quantity, some indefinite number of individuals within "the hardy hundreds". Similarly, in:

- (8) ... *there was no sound or light in the entire house to indicate that any of the occupants were awake.*

Judging by the evidence of *were*, we are to interpret *any* as evoking more than one from the group "the occupants". This intermediate scope of *any* in (7) and (8) can be contrasted with its sense in (9):

- (9) *If any of them has any power to veto the Secretary General's decision the nature of the organization will have changed.*

Here, if one can judge by the verb *has*, *any* expresses 'one, no matter which' from among the group constituted by "them". This minimal scope sense is sometimes made explicit by lexical means, as in (10):

- (10) *Turn off at any one of the marked picnic areas.*

Besides providing further illustration of the quantitative variation already noted, these sentences bring out clearly a second characteristic of *any*. In (7), (8), (9) and (10), the phrase following *any* evokes a group or set out of which *any* selects a sub-set made up of one or more individuals. That is to say, pronominal *any* designates a quantity as part of a whole which is often expressed by an *of*-phrase. In other words, *any* functions as a *partitive*.

It is important to keep this partitive function in mind if only because it helps us to understand the role of *any* in other contexts. For example, in:

- (11) *At each step of a derivation we look through all the transformations to see if any are applicable, apply any that we find are applicable and obligatory ...*

the first *any* has the intermediate sense of 'some', 'a certain number' out of "all the transformations". That is, we have a partitive function here as well, even though there is no *of*-phrase. Indeed, one could add *of them* to *any* here without substantially changing the sense.

The second *any* in (11) expresses something like 'all those', and so would appear to evoke maximum scope, that is, the totality of those "that we find are applicable and obligatory". Can we

properly speak of a partitive function in a case like this? I think we can because *any* here evokes all the entities in a set, the totality of parts making up a whole. This is a significant point because it tells us that pronominal *any* can function as a partitive whatever its quantitative sense:

- maximum scope would correspond to all parts;
- intermediate scope would correspond to a number of parts;
- minimal scope would correspond to a single part.

We are now in a position to characterize pronominal *any* with a little more precision. It is not merely a quantifier, but rather a *partitive quantifier* since it evokes a quantity or scope as a portion, more or less extensive, of a whole. In other words, *any* evokes a quantity as one term of a part-whole relationship.

How about adjectival *any*? Can it be realistically considered a partitive quantifier? In examples (1) and (2) above I think it can, because the very meaning of the sentence is to select one out of a larger number, one part out of the whole. Similarly for examples (3) and (4) where, although there is no idea of a choice since *any* has a 'maximum scope' reading, there is the idea of including all the individuals in the set, all the parts of the whole, however many or few they may turn out to be. Likewise in (5) and (6), the effect of *any* is clearly to set off one portion as opposed to another - what has been called a contrastive use. Thus in

- (5) *If any words and phrases are formulaic, they are.*

a portion of "words and phrases" is considered as possibly formulaic, thereby distinguishing certain words from the rest, which are, by implication, not formulaic. Such observations permit us to characterize *any*, whether in pronominal or adjectival usage, as a partitive quantifier.

So far we have been looking at examples involving substantives expressing discrete or 'count' notions. The two characteristics of *any* discerned up to this point - quantitative variation and partitive representation - are also found where substantives expressing continue or 'mass' notions are concerned. Thus in:

- (12) *Confiscate any alcohol you find.*

any, with maximum scope, evokes all the alcohol found, whatever the amount. Similarly in:

- (13) *Does this suggest ... that the UN is making any progress whatever toward its goal?*

the writer seems to have in mind something like 'even the slightest progress', i.e., a minimum scope, as opposed to what someone else might have claimed or even expected under the circumstances. Finally, in:

(14) *I don't think he got to drink any of it.*⁴

any with intermediate scope evokes some drinkable portion of whatever liquid "it" refers to. Such examples show that *any*, whether used as a pronoun or an adjective, can evoke a quantity as part of a whole, be the whole represented as 'count' or 'mass'.

Viewing *any* in this way - as a partitive - helps to clarify an important theoretical point about the nature of the limits imposed on the underlying movement of quantification. It can now be seen that the maximum scope of *any* is determined in a given sentence by the size of the whole or set of which it evokes the totality. That is to say, *any* can evoke a quantity just as great as, but never greater than, that of its substantive simply because part or parts cannot be greater than the whole. At the other extreme, *any* must evoke at least a minimal quantity (one for 'count' notions, and the smallest imaginable amount for 'mass' notions) because no quantity at all would not constitute a part. That is to say, the limits we have recognized in observing various uses of *any* are by no means arbitrary; they are a necessary consequence of the postulate that *any* is by nature a partitive.

The claim that *any* evokes a quantity as part of a whole helps us to understand its functioning in a number of uses. One of the most common uses of *any* is the expression:

(15) *in any event*

with the sense of 'whatever happens'. Generally used to sum up a discussion during which a number of eventualities have been suggested, this expression evokes the prospective realization of one happening or set of circumstances among all those envisaged, a sense made possible by its partitive nature, which permits *any* to evoke one possibility from among a set of possibilities. Expressions like *in any case* and *at any rate* also illustrate this use of *any* with minimal scope.

The functioning of *any* in a negative sentence can be shown by the following:

⁴ Lars Hermerén has pointed out that if *any* is stressed, it will have the sense of 'even the slightest bit', suggestive of minimum scope. This observation calls attention to another facet of *any* whose examination would be too lengthy to undertake here.

(16) *There isn't any Santa Claus.*

This might be said by some disappointed youngster who had expected to find a Santa Claus in a store. *Any* permits him to evoke the possibility of finding him whereas the negation denies this possibility access to reality. (16) may, however, have another sense. It might also be said by someone discovering that Santa Claus is not a real person. In this case, "Santa Claus" is thought as a proper noun, as a substantive whose extension is limited to one individual. That is to say, the whole or set presented by the substantive is made up of one indivisible part. This single part which constitutes the whole is evoked by *any*; when it is negated, it is the very existence of the individual and the set that is denied.

The following sentence provides another negative context:

(17) *He doesn't lend his books to anybody.*

Read with falling intonation on *any*, the sentence suggests that the speaker lends his books to nobody. That is, a single possible borrower is evoked by *any* as typical of all possible borrowers. When negated, it has the effect of denying his lending to any person whoever he may be. On the other hand, if *any* is given a fall-rise tone, the sentence expresses another meaning: 'he doesn't lend his books to everybody, just to certain people'. In this case, *anybody* has maximum scope. The negation denies this all-inclusive scope, but not narrower scopes, so the sentence implies that the subject does lend his books to certain people.

Since it is time to conclude, let me summarize by answering the question, or rather the questions, asked by the title of this paper:

Can Any Express any Quantity?

Read without stress on *any*, the question calls for the answer "yes" because it amounts to asking if *any* can express quantity, if *any* is a quantifier. On the other hand, if *any* is stressed, it evokes the possibility of all the different quantities imaginable, *quantity* here expressing a 'count' notion. In this case, although we have seen that *any* can express a variety of quantities, we must answer "no" because *any*, being a partitive, cannot express a quantity greater than the whole evoked by the substantive, or smaller than the minimal part involved in the whole. However, *any* can evoke any quantity within these limits because its potential meaning is a mental movement which can be intercepted anywhere between its beginning and its end.

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THE INTERJECTION NA IN GERMAN: A FUNCTIONAL ANALYSIS

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The particle na is one of the most widely used morphemes in the German Umgangssprache, ie. the everyday language used in all but the most formal speech situations.¹ Wängler who counted over 160,000 words of the Umgangssprache lists na on place 176, together with the verb machen.² However, in spite of this high frequency, we know very little about where, when, and how na is used. Grammars like Curme and Duden or dictionaries like Wahrig list some of the meanings of na and provide a number of examples, but this information, though interesting, is even more inadequate than the usual dictionary entry since the various meanings of na are largely determined by suprasegmental features like intonation, pitch,³ and length which cannot be conveyed by the traditional entry.³ Thus we are lead to believe that there are no important differences between the na's in sentences like 1 to 3 below:

1. Na? Wann kommt er? ____, when is he arriving?
2. Na! Stör mich nicht! ____, don't disturb me!
3. Na, die wird sich ärgern. ____, she'll be mad.

In this paper, I intend to provide some insights into the role that na plays within the communication process, and I will investigate the linguistic means that are used to achieve these goals. It will be shown that na not only serves to convey the affective attitude of the speaker, but that it is also used to sum up a situation and to signal the speaker's illocutionary intend.

Modern grammars and dictionaries generally classify na as an interjection. Syntactically, interjections are defined as independent linguistic elements with sentential functions.⁴ That is, even if used in connection with another sentence as in examples 1 to 3, they remain syntactically independent. In German this claim is supported by the fact that interjections do not trigger subject-verb inversion as will an adverb like dann "then" (ex. 4-6).

4. Na, ich werde ihn mal holen. ____, I'm going to get him.
5. *Na, werde ich ihn mal holen.
6. Na, dann werde ich ihn mal holen.

Na can occur alone or precede all major sentence types (ex.1 to 3). However, due to its independent status, it cannot be integrated syntactically into another sentence though we will see later that such an integration is possible at the phonological level.

Functionally, interjections are particles that, according to Curme, are used to "give vent to some sudden outburst of feeling or passion, or to give expression to some intimation of will, or on the other hand to imitate some sound in nature."⁵ Though *na* is not a sound of nature and is rarely used in a "sudden outburst of feeling or passion", it definitely is used to indicate the speaker's affective attitude. As a matter of fact *na* conveys such a wide and subtle range of emotions, needs, and demands that in most instances they can only be paraphrased with difficulties. In addition, however, *na* is also used to signal the illocutionary force both of the sentence it represents and of the sentence it precedes, eg. 7 and 8:

7. *Na?*

8. *Na? Kommt er? _____, is he coming?*

Besides these two major functions, *na* also serves to sum up a situation and/or to signal a change in topic.

9. *Na, dann will ich mal einen _____, I'll just make some coffee. Kaffee kochen.*

The *na* in 9 is used to sum up and simultaneously dismiss the preceding situation. It therefore appears that even though *na* is classified as an interjection, it has a much wider functional range than is generally attributed to these particles.

The question now arises how this range of meanings is signalled in a one-syllable word. Since syntactic means are excluded, only morphological and phonological ones remain. *Na*, like other true interjections, cannot undergo any derivational or inflectional processes except for one: under certain circumstances redublication is possible, for instance in 10:

10. *Na na, nun mal langsam! _____, slow down!*

Apart from this one instance, however, the whole functional load is carried by phonological means, and more specifically by suprasegmentals like intonation contour, pitch level, segmental length, and absence or presence of a pause.⁶ In order to discuss in detail how these phonological features relate to the various meanings of *na* I will distinguish between three basic representations based on the three major intonation contours.

- (1) *na* pronounced with a rising intonation contour, henceforth *na R*.
- (2) *na* pronounced with a level intonation contour, henceforth *na L*.
- (3) *na* pronounced with a falling intonation contour, henceforth *na F*.

Na R is used in the context of questions and *na F* in the context

of certain types of statements and commandes. Na L, which is relatively rare, mainly occurs if na is incorporated phonologically into the following utterance thus giving up its independent status as an interjection.

All three basic versions of na can occur alone or before another utterance. In the latter case, several restrictions are imposed on the types of combinations allowed. Thus na R can usually only precede a question, and not a command or a statement as in 11.

11. *Na? Ich muß jetzt gehen. ____, I have to go now.

In 11, the illocutionary intend of na R and of the following statement clash, producing an unacceptable combination. Thus na, if occurring with another sentence, has to signal the same illocutionary force as that sentence. And while questions, commands or statements use various means to signal their illocutionary force, na is restricted to intonation contours.

Besides signalling the illocutionary force of the following utterance, na R can also function to indicate that the speaker is re-opening a topic both s/he and the addressee are familiar with, or that the speaker is referring back to a situation that is known to both of them. This fact becomes clear when we compare identical questions, one with and one without na R.

12. Kommt Peter mit? Is Peter coming along?

13. Na? Kommt Peter mit?

Questions like 12 and 13 are not used in the same speech situation. While 12 is a straightforward neutral question, 13 signals the resumption of an interrupted conversation. The na indicates that Peter's coming along was discussed earlier. It also indicates that the discussion was interrupted, possibly for the addressee to procure additional information. The na in 13 could be paraphrased by Did you find out? and Tell me!. The same interpretation can be assigned to na R when it occurs without a follow-up question (ex. 7). Na R used in isolation is the reduced form of a more complex question which can be deleted since all partners in the exchange know what is meant. For instance, let us assume that two persons are in a room and the door bell rings. Person A goes outside to check and returns after a while. In this situation, B may very well ask: Na? and both of them know that a more explicit question has been omitted, namely 14:

14. Na? Wer war das? ____, who was it?

Na R in isolation therefore actually replaces a question, it functions as a type of sentence pronoun, tying into something that was previously said or done.

The emotional overtones that can be signalled by na R are

restricted to one basic dimension, namely that of expressing the attitude of the speaker towards the addressee. The degree of friendliness or good-will of the speaker can be measured by the length of na R. In the friendliest version, the intonation contour of na R actually changes to a falling - rising one like in 15.

15.

The final pitch of this contour is higher than the initial one. Moreover, the vowel is, by necessity, lengthened. The predominant meaning of this version of na R is not Did you find out? and Tell me! but rather something like: How are things going?

While the rising intonation curve of na R indicates a question, the falling contour of na F is associated with statements and commands.

16. Na! Komm schon! _____, come now!
17. Na, wenn das man klappt. _____, if that works out.
18. Na, wenn du meinst. _____, if you think so.

Like na R, na F can fulfill more than one function. However, while the main function of na R is that of signalling or asking a question, the basic function of na F is to indicate the affective attitude of the speaker. Secondly, under certain conditions, na F like na R can be used to indicate that the speaker is returning to a topic after an interruption, simultaneously dismissing that interruption. But it appears that contrary to na R, na F does not play an important role in signalling the illocutionary force of the following utterance. In other words, there is no clear-cut marking in na F that will unambiguously communicate that the following utterance is a command or a statement. Nevertheless there are certain patterns that will create specific expectancies in the listener. A na signalling surprise usually precedes a statement, while a na signalling annoyance will probably precede a command or a warning. Na F preceding commands is short and clipped, expressing the irritation of the speaker, for instance 19:

19. Na! Gib mir das Buch! _____, hand me the book.

It is interesting to note that, in the context of imperatives, na does not mean I order you to do something, but rather: I order you to stop what you are doing. A more complete form of na F preceding commands is therefore Na! Las das! "___, stop it!" and 20 is a more complete representation of the meaning of 19.

20. Na! Laß das und gib mir das Buch, stop that and hand me the book.

A variation of the above-discussed na F is found in the reduplicated form na na'. In this version, the first na is pronounced on a low tone, while the second na jumps to a higher level, is held, and then drops slightly, e.g.

21. Na na! Is jetzt! _____. Eat (now)!
22. Na na! Laß das und geh ins _____. stop that and go to bed!
- Bett!

This version of na F, though expressing a warning, is definitely less harsh than the one in 19 and 20. Typically, sentences like 21 and 22 are used when addressing children. Na na not only implies that the speaker wants the addressee to change his or her behavior but it also signals the friendly attitude of the speaker towards the addressee and is therefore the equivalent to na R with the falling-rising intonation in example 15.

While the short, clipped version of na F expresses irritation and impatience, a longer, more drawn out version of na F is used to signal doubt, surprise, and expectations. For instance, an emphatically lengthened initial n coupled with a medium pitch range is used by speakers to express expectations, mostly about the emotional reaction of other people (ex. 23, 24), or surprise (ex.25).

23. Na, die wird sich freuen. _____, she'll be pleased.
24. Na, ihr werdet euch wundern. _____, you're in for a surprise.
25. Na, das hätte ich nicht für _____, who'd have thought that
möglich gehalten. possible.

Na F with a lower pitch level and an even longer consonant is used to express doubt. The pitch level can be varied and the n lengthened depending on the degree of doubt that the speaker wants to express, e.g.:

26. Na, das ist ja noch einmal _____, that happened to work out
gut gegangen. alright.
27. Na, der kann uns viel _____, what does he know?
erzählen.
28. Na, wer das glaubt. _____, really?

This same meaning is also retained if na F of examples 23 to 28 occurs without follow-up sentence.

Na F, pronounced neutrally, i.e. with normal pitch level and consonant length, conveys a feeling of informality and familiarity in initial contact situations. At the same time, the use of na F in this context indicates that the speaker is now turning to the addressee, dismissing what went before, e.g.:

29. Na, Frau Müller, dann _____, F. M., come on in.
kommen Sie mal.
30. Na, Herr Meyer? Was kann ich _____, H.M.? What can I do for you
heute für Sie tun? today?

When discussing the use of na R it was shown that it can also serve to signal the resumption of an interrupted event (ex. 13 and 14). Na F can fulfill similar functions, e.g. 31 and 32:

31. Na, ich will Willi mal ____, I'll give Willi a call.
anrufen.
32. Na, dann woll'n wir mal los. ____, let's get going.

Curme claims that *na* in this context is used "when one forms unwillingly a resolution which he cannot well avoid."⁷ In my opinion this analysis is incorrect for *na* in 31 and 32. Rather, in these instances, *na* serves to sum up and/or dismiss what was said or done before.⁸ For instance, 31 could occur at least in two types of speech situations. One, A and B discuss for a while what to do. Finally, one of them sums up the discussion by using 31. Alternatively, A tells B that she wants to call Willi. Then they are interrupted. Now, by using *na* in the context of 31, A is dismissing the interruption and returning to the old topic. Similarly, *na F* is used in a narrative to summarize and to indicate resumption of the main topic after a digression, e.g. 33:

33. Gestern sind wir zu meiner "Yesterday we drove to my
Freundin gefahren, um unseren friend's place to pick up our
Hund abzuholen. Das hättest dog. You should have seen
du sehen sollen! Zuerst hat what happened! First he hid in
er sich im Garten versteckt, the yard, then ... and then ..,
danndanach....Na, finally we found him and were
schließlich haben wir ihn able to take off."
gefunden und konnten
abfahren.

Compared to *na R* and *na F*, *na L* with a level intonation contour is relatively rare. One of its main functions is to indicate appeasement, e.g. 34 and 35:

34. Na, so schlimm ist das doch ____, it's not that bad.
nicht.
35. Na, wer wird denn gleich ____, don't be such a crybaby.
weinen.

In this function, the pitch level of *na* is extremely low and the vowel, not the consonant, is emphatically lengthened. The longer the vowel and the lower the pitch, the more empathy the speaker is conveying. As was the case with the other manifestations of *na*, the *na* of appeasement can occur in isolation due to its distinctive phonological markings.

In all of the examples presented above, it was assumed that *na* is separated from the following sentence by a pause, indicated in writing by a comma, a question mark, or an exclamation mark. The pause indicates that *na* is not considered part of the following sentence but has independent status as an interjection. This claim is supported by the sentence intonation used with *na* and by the syntactic structure of the following sentence. However, *na* can give up this independent status and can become phonologically, though not syntactically, part of the sentence it precedes.

- (1) Na expresses the speaker's affective attitude, e.g. annoyance, impatience, friendliness, doubt, surprise, appeasement, and resignation.
- (2) Na indicates the illocutionary force of the sentence it replaces and of the sentence it precedes.
- (3) Na sums up a topic or situation, signals the return to an established topic, and dismisses an interruption.

All of these functions are expressed by fully exploiting a limited number of phonological means like intonation contour, pitch, quality of voice, segmental length, and pause phenomena. Most versions of na, due to their distinctive phonological markings, can occur alone without follow up sentence. In these instances na either replaces general utterances like: Did you find out?, Tell me!, How are things going?, and I order you to stop what you are doing! or very specific ones like Wer war das? (ex.14). In the latter case, na acts more like a sentence pronoun than an interjection.

It was said earlier, that the illocutionary force of na has to be identical to that of the utterance it precedes. This implies that na R occurs with questions while na F occurs with commands or statements. However, under specific conditions na F can accompany a question, e.g. 47:

47. Na! Was soll denn das?! ___, what's going on?

In 47, na F signals a command, while the following sentence is, at least formally, a question. However, in German certain questions are used and interpreted as commands rather than as quests for information and the occurrence of na F reflects this function. As a matter of fact, na R does not seem appropriate in the context of 47, since the sentence expresses irritation which cannot be captured by na R due to its rising intonation pattern which is generally associated with friendliness. The acceptability of 48 below is therefore questionable.

48.? Na? Was soll denn das?

Examples 47 and 48 show again that na and the sentence it accompanies are very closely linked. They have to signal identical illocutionary intent as well as identical affective attitudes.

There is one other question that should be discussed in this context. Consider the following two sentences.

49. Na, das ist doch unglaublich! ___, that's incredible.
50. Das ist doch unglaublich!

Both sentences signal the speaker's irritation and negative surprise, and in spite of the presence of na, 49 is in no substantial way different from 50. So why use na? The reason seems to be the

following: In 49, na contains all the emotional overtones the speaker reaffirms in the following sentence. Consequently the addressee will not have to listen to the whole utterance before being able to interpret the affective position of the speaker. The one syllable, na, will have told him all about that position. Na, then, is often used as a quick way to inform the addressee about the speaker's attitudes and/or about the type of sentence s/he is going to produce. Thus the use of na simplifies the speaker's and the listener's tasks. It is easier to signal a certain emotion in the intonation pattern of one syllable than to maintain this pattern over a longer utterance. For instance, in 49 na expresses indignation and irritation. If na is not present, as in 50, the speaker's indignation only becomes apparent at the end of the sentence when the content of the adverb unglaublich and the suprasegmentals combine to create that meaning. Na, then, is used by the speaker like a little flag which s/he waves saying: this is the kind of utterance I am going to make and this is how I feel about it.

In conclusion, it has been maintained in this paper, that the interjection na plays a multi-functional role in the communication process of German, a role which goes far beyond that generally assigned to interjections. Further research will probably show that the functional load of na is even greater than has been assumed here, especially in the area of turn-taking in conversations, and of continuing an established topic or introducing a new one.

NOTES AND REFERENCES

1. Historically, *na* appears to have developed from an OHG question particle *na*. However, this particle did not occur in sentence initial position. *Na* as an interjection is first attested in the sixteenth century. Cf. Friedrich Kluge, Etymologisches Wörterbuch der deutschen Sprache, 18th ed., (W. de Gruyter & Co., Berlin, 1960) p. 498.
2. Hans-Heinrich Wängler, Rangwörterbuch hochdeutscher Umgangssprache, (N.G. Ebert Verlag, Marburg, 1963), p. 50.
3. George O. Curme, A Grammar of the German Language, (F. Ungar Publ. Co., New York, 1964), Duden: Grammatik der deutschen Gegenwartssprache, 2nd ed., (Dudenverlag, Mannheim, 1966), and Gerhard Wahrig, Grosses deutsches Wörterbuch, (Lexikon Vlg., Berlin, 1966).
4. See Henning Bergenholtz and Burkhard Schaefer, Die Wortarten des Deutschen, (Vlg. Klett, Stuttgart, 1977), pp. 140-41.
5. Curme, p. 400. (note 3 above)
6. For a discussion of the use of intonation for the realization of expressive systems see Gillian Brown et al., Questions of Intonation, (University Park Press, Baltimore, 1980), p. 14. For German intonation patterns see Otto van Essen, Grundzüge der hochdeutschen Satzintonation, (A. Herm Verlag, Düsseldorf, 1964).
7. Curme, p. 401 (note 3 above)
8. This specific meaning of *na* is emphasized by the use of *dann* and *mal* or at least by one of them. If both elements are dropped, the ensuing utterance appears odd if not unacceptable.
 - a. Na, ich will mal anrufen.
 - b. Na, dann will ich mal anrufen
 - c. Na, dann will ich anrufen.
 - d.? Na, ich will anrufen.

VI

DISCOURSE STUDIES

ON TYPOLOGY OF LEXICAL COHESION IN DISCOURSE

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The paper is a comparative study of textual cohesion in English and Russian. In existing linguistic literature, as well as in normative grammars, if a comparison between the two languages is made in this respect, it is usually to point out those differences that are part of the structure, e.g. the definite article, or the substitutes such as 'one' and 'do' in English and absence thereof in Russian. On the other hand, in works that are concerned with the calculus of cohesive ties and describe either English or Russian, one finds certain items and processes that are common to both languages. Moreover, a classification developed for the description of one language may be applied to a study of the other (Halliday 1976; Simmons 1981). Inadvertently, one gets an impression that when the same text categories are found in two languages, both use them in the same way.

The purpose of this paper is to compare English and Russian in one area - namely, re-entry of the same object or event in the text. It considers only those items and operations of cohesion that are similar in both languages and may be used in the same situations.

The framework of this study may loosely be called a "grammar of choice" since rules and regularities that I strive to discover can be formulated as tendencies or probabilities rather than deterministic laws. A "grammar of choice" study cannot rely on contrived examples; my conclusions will be based on the comparison of existing English and Russian original texts and their published translations - by different authors and translators. My objective is to examine when and how each language, given the same choice of cohesion devices, actually employs them.

In their book Cohesion in English, Halliday and Hasan (Halliday 1976) differentiate five types of cohesion in English: Reference, Substitution, Ellipsis, Conjunction, and Lexical Cohesion. Three of these will be considered here: reference, ellipsis and lexical cohesion. Of the remaining two, conjunction has no direct bearing on the topic of this paper, since conjunction establishes logical relations between events and operates between sentences. Since substitution per se is not found in Russian, it will not be examined separately, but only in conjunction with ellipsis or lexical cohesion.

Reference is deictic by nature: the item which is used to identify an object or an event can only be interpreted through information found elsewhere in the context. Both English and Russian have at their disposal deictic signs with similar combinations of indexical and symbolic components (terminology of Burks 1949), from purely indexical he, they, this, that to those containing various symbolic components as in here 'this place', there 'that place'.

so 'in this manner', etc. In case of Ellipsis the omitted information is also retrieved from the accompanying text. While reference is a semantic relation of the deictic sign to its antecedent, ellipsis is primarily grammatical; the omission presupposes a word of the same grammatical class. However, similarly to reference, ellipsis points to another item elsewhere in the text, and in this sense, it is also deictic. Lexical Cohesion is a matter of vocabulary. The tie between two items is based on their intrinsic relation through meaning plus their co-occurrence in the same text. All or part of the meaning of the first item is repeated in the second item; the repetition may range from maximum (lexical re-iteration) to partial (synonyms, near-synonyms) to minimum (the re-entry is a general word like man, thing, matter).

All three types of cohesion exist in both English and Russian. The two opening examples below are cited to establish a common ground from which to proceed to further discussion; they show that both languages may occasionally choose the same means of cohesion.

1. Presently Tom checked his whistle. A stranger was before him - a boy a shade larger than himself. ... This boy was well dressed too - well dressed on a weekday. ... He had shoes on - and it was only Friday. He even wore a necktie, a bright bit of ribbon. He had a citified air about him that ate into Tom's vitals. (Mark Twain)
 Vdrug Tom perestao svistet'. Pered nim ('him') stoyal neznakomec, mal'čiška čut' pobol'she ego ('him'). ... K tomu že na mal'čike ('boy') byl narjadnyj kostjum - narjadnyj kostjum v budnij den'! ... Na nogax u nego ('he') byli bašmaki, darom, čto segodnja ešče tol'ko pjatnica. U nego ('he') byl daže galstuk - očen' jarkaja lenta. Voobšče on ('he') imel vid gorodskogo ščegolja, i èto vzbescilo Toma.
2. Vposledstvii ... raznye učreždenija predstavili svoi svodki ('reports') s opisaniem ètogo čeloveka ('this man')... v pervoj iz nix ('first of them') skazano, čto čelovek ètot ('this man') byl malen'kogo rosta, zuby imel zolotye i xromal na pravuju nogu. Vo vtoroj ('second') - čto čelovek ('the man') byl rostu gromadnogo, koronki imel platinovye, xromal na levuju nogu. Tret'ja ('third') lakoničeski soobščaeť, čto osobyx primet u čeloveka ('the man') ne bylo. Prihoditsja priznat', čto ni odna iz ètix svodok ('these reports') nikuda ne goditsja. (Bulgakov)
 Afterwards ... various official institutions filed reports describing this man. ... the first says that the man was short, had gold teeth, and limped on the right foot. The second, that the man was of enormous height, had platinum crowns, and limped on the left foot. The third states laconically that the man had no special distinguishing characteristics. We must discard all these reports as quite worthless.

In example 1, re-entry for both actants, Tom and the strange boy, coincide throughout the paragraph, with the only exception of the omitted demonstrative in the Russian translation of this boy in the

third sentence, where the definitization is effected through the thematic position of the noun mal'čik in the sentence. (A translation with the demonstrative ètot 'this' would also be possible). Both the original and the translation re-introduce the name Tom in the last sentence to avoid ambiguity. Example 2 contains lexical repetition in all re-entries of 'man', and three instances of ellipsis of the referent 'report', plus one repetition of this word in the last sentence. The English anaphoric demonstrative this and the definite article the can both be rendered in Russian either by a demonstrative, or by the thematic position in the sentence without any definitizer. These limitations are structural, and should not concern us here. Structural peculiarities aside, both languages have similar options in selecting a cohesive tie.

A further analysis of texts and translations, however, shows that English and Russian display different preferences in choosing an item for re-entry. Russian tends to be more repetitive than English, while the latter prefers variation. Russian often employs lexical cohesion where English selects reference or ellipsis. Russian likes absolute and descriptive repeated items, and English chooses relative, and therefore deictic, reference. In all examples that follow, the translators could have used cohesive types similar to those in the original without producing ungrammatical sentences. Examples will be arranged according to the type of cohesion in the English text, no matter whether it is an original or a translation, in this order: reference (personal, demonstrative, comparative), ellipsis, lexical cohesion. The original, of course, always comes first.

Personal reference is signified by the personal pronouns and possessive determiners. Since first and second persons are primarily empirical and situational, it is the third person pronominalization that is extensively used for textual cohesion. In examples 3, 4, 5 and 6 below, lexical re-iteration in Russian is not dictated by structural considerations: no ambiguity would result if personal pronouns - 'exact' equivalents of the English ones - replaced the repeated noun.

3. The old lady pulled her spectacles down and looked over them about the room; then she put them up and looked out under them. She seldom or never looked through them for so small a thing as a boy: they were her state pair, the pride of her heart, and were built for "style", not service...
(Mark Twain)

Staruška spustila očki ('spectacles') na končik nosa i ogledala komnatu poverx očkov ('spectacles'); potom vzdernula očki ('spectacles') na Tob i gljanula iz-pod nix ('them'). Ona redko smotrela skvoz' očki ('spectacles'), esli ej prixodilos' iskat' takuju meloč', kak mal'čiška, potomy što èto ('this') byli ee paradnye očki ('spectacles'), gordost' ee serdca; ona nosila ix tol'ko dlja važnosti...

4. - Sir, about Enid?
- What about her? (Gore Vidal)

- Šer, ja xotel sprosit' vas nasčet Inid.
- Čto nasčet Inid?

5. Voprosy ('matters') ljubvi i smerti ne volnovali Ippolita Matveeviča Vorob'janinova, xotja ètimi voprosami ('these matters') po rodu svoej služby on vedal s devjati utra do pjati večera ežednevno. (Il'f and Petrov)
Matters of life and death did not worry Ippolit Matveyevich Vorobyandinov, although by the nature of his work he dealt with them from nine till five every day.
6. I vot dva goda tomu nazad načalis' v kvartire ('the apartment') neob'jasnimye proisšestvija: iz ètoj kvartiry ('this apartment') ljudi načali bessledno isčezat'. (Bulgakov)
 Two years ago, however, a series of inexplicable events began to plague the apartment: people began to disappear from it without a trace.

Halliday and Hasan make the following comment about personal reference in English (Halliday 1976:306):

'Why do we refer to 'John' as *him* rather than as *John*? Because *John* is vague, whereas *him* is definite. *John* could be any old John; but *him* means 'that particular individual whose identity we have established and agreed upon'. We refer to John as *him* rather than as *John* in order to signal that his identity is a feature of the environment. And the same principle applies to the other reference items.'

The same idea, but with a somewhat different emphasis can be found in textbooks and manuals on English style and writing, where 'unnecessary repetition' is said to 'draw unwarranted attention' to the repeated word (Glorfeld 1980: 102), and the beginning writer is advised that 'the right way to avoid awkward repetition of a noun is to use a pronoun, and to rely on the pronoun as long as possible' (Daniel 1967: 104). Apparently, a re-iteration of the noun, especially the name, in English is felt as an interruption in the text continuity, because at that point some kind of change occurs in the situation, or else, repetition is perceived as stylistically marked. No such interruption or markedness is expressed in Russian, where re-iteration can be (although not always) stylistically neutral, as in 7 below. It is interesting to note again that the translator substituted pronouns for several 'unnecessary repetitions' of the name in the Russian original.

7. Ivan trjxnul golovoj, ubedilsja v tom, što ona ne bolit, i vspomnil, što on naxoditsja v lečebnice. Èta mysl' potjanula za soboj vospominanie o gibeli Berlioza, no segodnja ono ne vyzvalo u Ivana sil'nogo potrjasenija. Vypavšis', Ivan Nikola-evič stal pospokojnee i soobražat' načal jasnee. Poležav neko-toroje vremja nepodvižno ..., Ivan uvidel knopku zvonka rjedom s soboj ... (Bulgakov)

Ivan shook his head, discovered that it did not hurt, and remembered that he was in the hospital. This thought brought with it the memory of Berlioz' death, but now it did not cause him violent shock. The long rest calmed Ivan and he began to think more clearly. He remained motionless for a time ... then he saw the bell button near him.

Demonstrative and comparative reference also exists in both languages, but is more typical of English.

8. bufetčik ... uznał, čto Kuz'min živet bukvall'no čerez dvor v malen'kom belen'kom osobnjačke ('house, mansion'), i minuty čerez dve byl v ètom osobnjačke ('this house').
(Bulgakov)
the bartender... (learned) that Kuzmin lived literally across the yard, in a small white house. Two minutes later he was there.
9. "Why, I never see anything like it. What did make him act so?"
"Deed I don't know Aunt Polly; cats always act so when they are having a good time."
(Mark Twain)
- V žizni svoej ne vidala podobnyx čudes ('such marvelous, incredible things'). S čego èto on tak ošalel ('went so crazy')?
- Pravo že ne znaju, tetja Polli. Koški vseгда kувyrkajutsja, ('somersault'), kogda u nix kakaja-nibud' radost'.

Taken out of the context, the English dialogue in 9 resembles an algebraic formula in which the variable act so can assume any value, i.e. can stand for any action. The only information the reader gets from it is that it is a comparison (identity) with an action or actions described elsewhere in the text. Indeed the preceding half page is devoted to a description of how precisely the cat 'acted' when mischievous Tom Sawyer gave him the 'Painkiller': 'Peter sprang a couple of yards in the air, and then delivered a war whoop and set off round the room, banging against furniture, upsetting flowerpots, and making general havoc. ...' and so on. The Russian reader, on the other hand, has a fairly clear picture even without the context, although he, too, must refer back in order to interpret such items as podobnyx 'such' and tak 'so'. None of the underlined words in the Russian translation are re-iterations, rather, they are synonyms or near synonyms, sharing semantic components with other words in the preceding description.

Since ellipsis is as deictic as reference, it is to be expected that in place of the English ellipsis Russian might resort to lexical cohesion. In other words, where English omits information, Russian repeats it. The following three examples contain nominal, verbal and clausal ellipsis respectively; in all cases Russian repeats the word or phrase.

10. Teper' on byl jasno viden: usy-peryški, steklyško ('lens') pensne pobleskivaet, a drugogo steklyška ('the other lens') net. (Bulgakov)
Now he was clearly visible: a tiny feather mustache, one lens glinting in the pince-nez, the other missing.
11. He never got mad at anybody. People with red hair are supposed to get mad very easily, but Allie never did, and he had very red hair. (Salinger)
Nikogda on ne razozlitsja ('would not get mad'), ne vspylit. Govorjat, čto ryžie čut' čto - načinajut zlit'sja ('begin to be mad'), no Allie nikogda ne zlišsja ('didn't get mad'), a on byl užasno ryžij.
12. Voda v prude počernela, i legkaja lodočka ('boat') uže skol'-zila po nej, i slyšalsja plesk vesla i smeški kakoj-to graždan-ki v lodočke ('a woman in the boat'). (Bulgakov)
The water in the pond turned black, and a light boat was gliding over it. A woman giggled.

For the purposes of this paper it is irrelevant whether did in 11 is regarded as a PRO-verb substitute, or as a tense operator. It is relevant, however, that the direct description of the action, which is contained in the lexical verb, is not repeated in English, and a deictic sign is used instead. Grammars usually explain cases like 11 by systemic differences between English and Russian, namely by the lack of certain verbal operators in Russian which is compensated through the repetition of the lexical verb. While such an explanation holds true for a large number of cases, and may apply to 11, it does not account for examples 13 and 14 below. In example 13, English omits the lexical verb and repeats only the passive voice operator; in Russian, however, which also has a passive operator, it is not this operator that is repeated, but the lexical verb. The translator even changes the structure in order to re-enter the lexical verb alone.

13. ...Burden walked up the aisle to his seat, chin held high so that he would be recognized. He was. (Gore Vidal)
... on prošel po proxodu k svoemu kreslu, vysoko zadržav podbodorok, čtoby byt' uznannym ('to be recognized'). Ego uznali. ('"they"[impersonal] recognized him')
14. "I can lick you!" - Xočeš', ja tebjā vzduju! ('lick')
"I'd like to see you try it." - Poprobuj! ('try')
"Well, I can do it." - A vot i vzduju!
"No, you can't either." - A vot i ne vzdueš'!
"Yes, I can." - Zaxoču i vzduju!
"No, you can't." - Net, ne vzdueš'!
"I can." - Net, vzduju!
"You can't." - Net, ne vzdueš'!
"Can!" - Vzduju!
"Can't!" - Ne vzdueš'!
- (Mark Twain)

In dialogue 14 from Mark Twain's "Adventures of Tom Sawyer", the situation is set up at the beginning: two boys are about to start a fight. The conversation is of necessity laconic, and the crescendo of the quarrel is built on repetition. The modal operator can repeated throughout the English passage, each time refers to the content verb lick in the first line; in the translation, 'licking' is re-introduced in each line anew. Without the first line, the English dialogue expresses an argument in general, whereas the Russian dialogue is specific and interpretable at each point.

According to Halliday and Hasan, there are two types of lexical cohesion in English: reiteration and collocation. Reiteration implies that two words share a certain number of semantic components. The following example from Halliday and Hasan illustrates different degrees of identity in meaning arranged on a downward scale:

'I turned to the ascent of the peak.	The ascent	is perfectly easy.
	The climb	
	The task	
	The thing	
	It	
Here we have (1) the same item repeated, (2) a synonym, (3) a superordinate, (4) a general noun and (5) a personal reference item.' (Halliday 1976:279)		

This scale is continuous and not discrete, and there can be various intermediate degrees. Besides, the personal reference item at the bottom shows that there is no sharp division between types of cohesion: a general noun is on the borderline between lexical reiteration and deictic reference.

In Russian, all these sub-categories of lexical reiteration can be found as well. Our analysis shows, however, that Russian leans more towards the upper pole of the scale, with maximum repetition, while English prefers more general terms.

15. ..the other Senator was visibly enhanced in the eyes of his people because he was acquainted with the Defender of the Constitution. Burden made a mental note to cultivate the new man. (Gore Vidal)
... i avtoritet molodogo senatora ('senator') zametno podnimsja v glazax ego zemljakov, tak kak on znakom s samim Zaščitnikom Konstitucii. Myslenno Berden rešil vzjat' šefstvo nad novym senatorom ('new senator').
16. Na ogromnom snimke čerez vsju polosu bylo izobraženo mnogočislennoe semejstvo kozlotura ('goatibex') vmeste s junymi kozloturjatami ('goatibex's offspring'). (Iskander)
An enormous photograph depicting the goatibex along with his harem and young offspring ran the full width of the page.
17. He canvassed his system. ... he thought he could detect colicky symptoms, and he began to encourage them with considerable

hope. But they soon grew feeble, and presently died wholly away. (Mark Twain)
 On issledoval svoj organizm.... emu pokazalos', što u nego na-
 činaetsja rez' v živote ('abdominal pain, colic'), i on obra-
 dovalsja, nadejas', što boli ('pains') usiljatsja. No boli
 ('pains'), naprotiv, vskore oslabei i malo-pomalu isčezli.

In the first two examples above, Russian repeats the meaning completely (in 16 the word kozloturjatami does not only mean 'offspring', but specifies the parent kozlotur by the repetition of the morpheme), and English resorts to a general word or a superordinate. In the last example, Russian uses superordinates for both re-entries, and still makes them more repetitive and independently informative than the reference items them and they in the English original.

In the other type of lexical cohesion, collocation, cohesion is achieved through co-occurrence of two words which are related in some lexico-semantic way, e.g. as opposites (day - night; intelligent - stupid), or as members of an ordered series (Monday - Tuesday), or they may belong to the same "register" (school - teacher - students). Collocation plays a text-binding role in both languages, but here, too, Russian tends to be more repetitive in that it includes in the second word a greater amount of information already mentioned in the first.

18. About half past ten the cracked bell of the small church began to ring, and presently the people began to gather for the morning sermon. (Mark Twain)
 Okolo poloviny odinnadcatogo zazvonil nadtresnutyj kolokol malen'koj cerkvi ('church'), i prixožane ('parishioners') stali sobirat'sja k utrennej propovedi.

'The church' and 'people attending, or belonging to, this church' are likely to co-occur in the same text. In English, the general word people acquires its specific interpretation because the meaning 'church' is already established for the given situation in the first word. Russian repeats this meaning twice.

Another example:

19. Klavdija Ivanovna byla glupa ('stupid'), i ee preklonnyj vozrast ne pozvoljal nadejat'sja, što ona kogda-nibud' poumneet ('will have more sense'). (Il'f and Petrov)
 Claudia Ivanovna was stupid, and her advanced age gave little hope of any improvement.

The meaning component 'intelligence', upon which the anthonymic relation is based, is repeated in Russian in the second entry. In the English translation 'improvement' is an abstract concept of polarity, and the word improvement must be cointerpreted together with stupid.

Conclusion. Given the choice of the same means and devices for textual cohesion, English and Russian differ in their actual utilization of these possibilities. Russian in greater degree than English relies on lexical cohesion, especially on repetition of the same lexeme. Re-entry for the same referent in Russian is more often direct and absolute. English, on the other hand, tends to employ deixis; for the second entry it selects indirect and relative terms.

In English, it is sufficient to designate a substance, action or property once, and refer to it by "hints": indexes or abstract terms. The recipient retains the context in his memory and constantly refers to it either through pronominal or other reference, or through cointerpretation of general terms or by reconstructing the omitted information.

Russian expression is less dependent on the context; each time the entry itself provides enough information for independent and unambiguous interpretation. A complete (or nearly complete) description of the referent is repeated more frequently in lexical re-entries, and the recipient of the Russian text does not have to search for information as far back in the text. Typologically, then, the Russian text is characterized by greater informational redundancy than English.

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TOWARDS AN INTERACTIONAL GRAMMAR OF SPOKEN ENGLISH

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1. The context of an interactional grammar

Opposition to what many linguists view as the excessive abstraction of language away from speech in the transformational-generative paradigm has grown steadily during the past decade. Makkai argues that if linguistic theories are to avoid being merely intellectual games they ought to be about what human beings do. Halliday points out the dubious relevance of a linguistics in which most of the distinctions bearing on human performance are simply 'idealized out of the picture.' Rommetveit claims that if linguistics is to serve human understanding it must expand the scope of its analysis from the sentence in vacuo to the utterance in its specific setting of interpersonal communication. Anttila and others speak of saving linguistics as a human science (cf. bibliography Janney 1981).

The opposition is symptomatic of a shift of interest in linguistics away from the study of language as an autonomous code to the study of language in relation to what Kenneth Pike calls 'unified theories of human behavior.' The recent emergence of the term 'nonautonomous linguistics' (cf. Arndt 1981) further indicates the changing focus of linguistics, which Hymes called for more than a decade ago, from what is constant in grammar to what is said by actual speakers, and from what is innate in human nature to what is social in human linguistic interaction (cf. Arndt/Janney 1979b).

One of the problems in developing a theory of human linguistic interaction is the difficulty of accounting for the linguistic effects of 'humanness' and 'interactivity' on everyday verbal choices. Gebauer, Tyler, Itkonen and others (cf. Arndt/Janney 1981) share the view that linguistic interaction can hardly be explained adequately without referring to the roles of intention and convention in these choices. Speech, they claim, is the act of a willful ego constrained by the necessity of communicating with other members of his group. The fact that a speaker speaks in a social context (i.e., to another person) imposes certain constraints on what he may say if he wishes to be understood. At the same time, as a willful human agent, he brings experiences, attitudes, assumptions, intentions, feelings, abilities and so on to the context which influence his linguistic behavior within these constraints. Thus, the true locus of a theory of human linguistic interaction ought to be neither the homogeneous language

group as an arbiter of the rules of language, nor the individual speaker as a producer of linguistic variation, but the individual speaker within the group as a producer of rule-governed linguistic variation (cf. Arndt/Janney 1980).

Ervin-Tripp, Schegloff, Hymes, Bennett and others have long maintained that conversation is a structured linguistic activity and that it can be described with properly constructed linguistic models (cf. Arndt/Janney 1979b). Yet there remains an urgent need to integrate notions of intention and convention into such models as complementary concepts, and to make these models not only descriptive but also explanatory. To do this, it is necessary first to develop a practical means of identifying the functional determinants of what Ervin-Tripp calls 'structures of communicative choice' (cf. also Lenk on 'Entscheidungslogik', or the logic of speaker decisions, in Arndt/Janney 1981). Once this is done, we will be in a much better position to explain rule-governed linguistic variation in everyday speech.

A second difficulty in developing an adequate model of linguistic interaction lies in the cross-modal nature of conversation itself. Speech is the central activity of conversation; yet as Trager, Hall, Abercrombie, Scherer, Birdwhistell and others have shown, conversation is a multichannel communicative process involving both verbal and nonverbal signals in a highly structured relationship (cf. Scherer in Arndt/Janney forthcoming). An actual speaker is not limited to the communicative resources of the linguistic code alone. He expresses his thoughts, feelings and intentions by uttering verbal signals and modulating or modifying these with prosodic and sociokinesic signals. Message-related vocal cues and body movements are no less important in everyday speech than verbal signals. Research by Argyle, Mehraian, Wiener and others (cf. Arndt/Janney forthcoming) suggests that in some contexts nonverbal signals are even more important than verbal signals. The implication of these and other studies is that nonverbal behavior is linguistically meaningful - and thus theoretically important to a model of human linguistic interaction - whenever it is combined with verbal behavior for a better understanding of utterances in context.

2. Interactional grammar as a concept

An interactional grammar is a grammar of communicative choices in context. It describes patterns of functional variation in face-to-face speech using token utterance options modified by token prosodic and sociokinesic options. The grammar treats speakers' communicative goals and interpersonal attitudes as functional determinants of verbal, prosodic and sociokinesic choices. It provides procedures for describing ranges of potential choice in all three communicative modes, and procedures for interpreting patterns of choice in different interactional contexts.

Interactional grammar is based on the notion that the recurrence of communicative goals and speaking situations in everyday conversation leads speakers to develop repertoires of more or less conventional communicative strategies for expressing themselves in face-to-face contexts. The ability to communicate verbally and nonverbally across distances of age, experience, attitude, social class, group membership, power and so on permits speakers to enter as regular participants into socially and functionally distinct groups and to assume different roles in different situations. This idea, which is implicit in functional linguistic theory, is often associated with the language-centered work of the ethno-methodologists and the symbolic interactionists (cf. Sacks, Garfinkel, Goffman, Cicourel, Schutz and others in Arndt/Janney 1979b).

3. Communicative goals

The grammar is organized around a framework of communicative goals based on Bales' functional interaction categories, Soskin/John's utterance categories, Jakobson's and Halliday's discussions of language functions, and attempts during the past decade to construct functional taxonomies in applied linguistics (cf. Arndt/Janney 1979b).

Communicative goals (see Figure 1) are broken down into semantic domains (e.g., cognitive, affective and conative meaning). Semantic domains are subdivided into general categories of communicative intention (e.g., conative meaning: personal volition, interpersonal volition). Communicative intentions are subdivided into communicative content categories (e.g., interpersonal volition: suggestion, offer/proposal, promise, command, permission); and content categories are differentiated by communicative focus (e.g., offer/proposal: eliciting, giving, avoiding giving, accepting, declining). The result is a systematic overview of the types of meaning treated in the grammar. Each of the final categories is a separate unit in the grammar, containing a series of token utterance options related to the communicative goal. Token prosodic and sociokinesic options are provided for modifying and modulating the meanings of the token utterance options, and various patterns of verbal, prosodic and sociokinesic choice are interpreted.

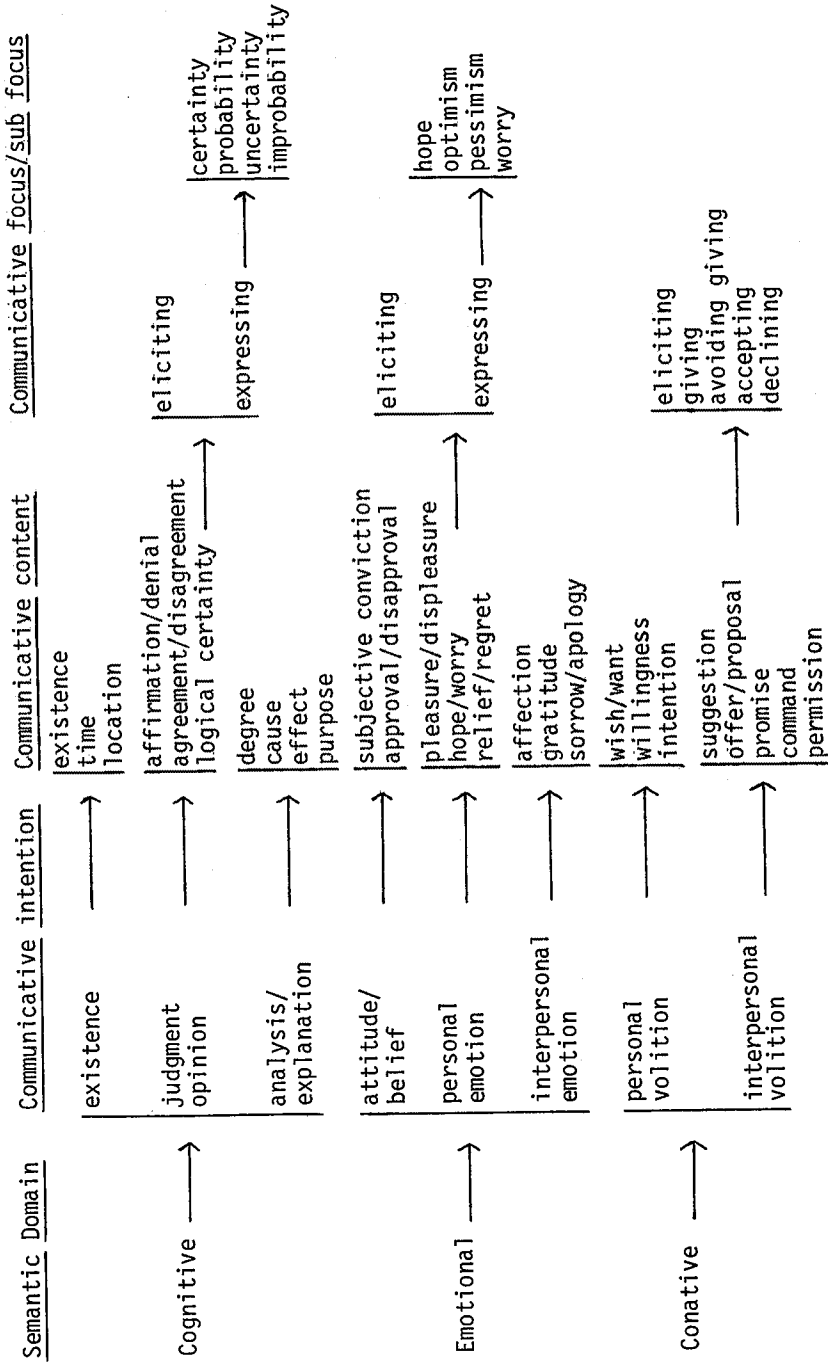


Figure 1. A preliminary overview of communicative goals

4. Dimensions of verbal variation

Members of all societies are expected to be able to use different registers or styles of speaking, and within these, to adopt different speaking strategies in different social situations. The token utterances provided in the grammar illustrate both types of variation, i.e., interstylistic and intrastylistic variation (see Figure 2). Three stylistic varieties are illustrated: ego-near, ego-neutral and ego-distant. Within each style, three strategic varieties are illustrated: assertive, noncommittal and nonassertive. Choices among stylistic varieties figure in the negotiation of social distance in everyday speech (cf. Joos, Crystal/Davy, Giles/Powesland, in Arndt/Janney 1980). Choices of verbal strategies within styles figure in the negotiation of interpersonal dominance/submissiveness and like/dislike (cf. Goffman, Ertel, Lenk, in Arndt/Janney 1981).

Inter-stylistic choices	Intra-stylistic choices	Token utterances
Ego-near	1	You're going to the meeting, right, John?
	2	Going to the meeting, John?
	3	Think you'll be going to the meeting, John?
Ego-neutral	1	I suppose you'll be at the meeting Mr. Jay, is that right?
	2	Will you be at the meeting, Mr. Jay?
	3	Are you planning to be at the meeting, Mr. Jay?
Ego-distant	1	You'll be attending the meeting, is that correct, sir?
	2	Will you attend the meeting, sir?
	3	Do you intend to go to the meeting, sir?

Figure 2. Verbal dimensions of conversational interaction:
Inter- and intrastylistic choices

(1) assertive; (2) noncommittal; (3) nonassertive

6. Dimensions of sociokinesic variation

Birdwhistell, Scherer, Ekman/Friesen and others (cf. Arndt/Janney forthcoming) point out the importance of message-related body motions in everyday speech. Three types of sociokinesic variation are illustrated in the grammar: (1) body posture, e.g., relaxed, alert, visibly tense, (2) facial expression, e.g., broadly smiling, smiling, unsmiling, frowning, and (3) head position, e.g., raised, level, lowered (see Figure 4). Choices of body posture signal the relative presence or absence of emotional charge in an utterance. Choices of facial expression act as interpretive cues to the direction of the emotional charge - e.g., positive or negative - signalled by pitch prominence and body posture (cf. Argyle et al. in, in Arndt/Janney forthcoming). Choices of head position act as interpretive cues to the speaker's interpersonal confidence or insecurity with respect to the listener (cf. Argyle et al., Wiener/Mehrabian, Mehrabian/Ferris, in Arndt/Janney forthcoming).

Token utterance	Body posture	Emotional charge	Facial expression	Direction of emotion/attitude	Head position	Interpersonal confidence
O.K., I'll go if you want me to.	relaxed	weak	broadly smiling	positive	raised	confident
	alert	strong	smiling		level	
	visibly tense		unsmiling	negative	lowered	insecure
			frowning			

Figure 4. Sociokinesic dimensions of conversational interaction: Body posture, facial expression, head position

7. The cross-modal interpretation of utterances in context

In line with Bolinger's comments on linguistic meaning (cf. Janney forthcoming), the grammar presupposes that utterances are complexes of facts, attitudes, emotions, and intentions reflecting speakers' orientations simultaneously towards what they are saying, why they are speaking, and the contexts (including their momentary group and interpersonal roles) in which they are speaking (cf. Arndt 1978). In order to interpret the cross-modal influences of verbal, prosodic and sociokinesic choices on the meanings of utterances in context (see Figure 5) it is necessary to postulate what Berger/Luckmann (cf. Arndt/Janney forthcoming) call a 'sincerity condition', and assume speakers are not intentionally trying to be misleading by issuing false signals.

CONTEXT SIGNAL	FACE-TO-FACE CONVERSATION			WRITTEN COMMUNICATION
	Ego-near	Ego-neutral	Ego-distant	
<u>VERBAL</u> (lexical and syntac- tic signals)		Verbal signals		
<u>PROSODIC</u> (nucleus pitch placement, prominence and direc- tion)				
<u>SOCIOKINESIC</u> (facial expressions, head and body signals, etc.)				

Key: ----- = verbal signals

———— = prosodic signals

----- = sociokinesic signals

Figure 5. The relative importance of verbal, prosodic and sociokinesic signals in various interactional contexts.

Work on the cross-modal interpretation of utterances in context has progressed significantly in recent years. The earlier interpretive systems used by Trager, Hall, Abercrombie and others have since been refined by Birdwhistell, Scherer, Ekman and Friesen (cf. Arndt/Janney forthcoming). Research by language-

centered social psychologists like Argyle, Mehrabian, and Wiener has been especially important to explaining interdependent verbal and nonverbal communicative patterns in face-to-face interaction (cf. Arndt/Janney 1980).

There is general agreement, at least among social psychologists, that nonverbal communicative behavior is primarily - if by no means exclusively - interpersonal. Studies of the verbal and nonverbal communication of interpersonal attitudes and emotions suggest that the interpretation of affective meaning plays a larger role in linguistic interaction than is normally assumed by linguists. The importance of nonverbal interpretive cues appears to increase proportionately to the ego-nearness of the speaker and listener. The zone of semantically relevant nonverbal communicative behavior decreases as speaker-listener relationships become more formal and their language begins more to resemble written communication. In other words, the greater the informational burden placed on lexico-syntactic signals, the less the interpretive weight placed on nonverbal signals. Nevertheless, in cases where verbal messages are contradicted by prosodic and sociokinesic signals - e.g., 'That hat you're wearing is just terrific!' accompanied by extreme pitch nucleus prominence, a frown, and a raised head - nonverbal signals decidedly override verbal signals.

8. Interpretive projections

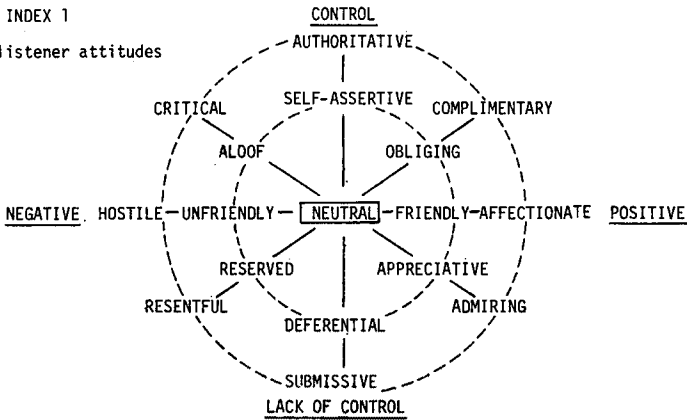
Each unit of the grammar interprets patterns of verbal prosodic and sociokinesic choice in different interactional contexts. Attitude indices such as those in Figure 6 provide the systematic basis for interpretive projections. A distinction is made in the indices between the emotional and volitional components of attitudinal states, and the indices define attitudes with respect to three different types of object:

- (1) speaker-listener attitudes,
- (2) speaker-subject attitudes (reported events), and
- (3) speaker-subject attitudes (projected events).

The emotional and volitional components of attitudinal states have been consistently identified but variously labelled by psychologists and social psychologists (cf. Leary, Gough, Krech/Crutchfield, Brown/Gilman, Conte/Plutchik, in Arndt/Janney 1979b, Osgood, in Arndt/Janney 1979a). The horizontal axes of the indices represent speakers' positive or negative emotional responses towards the objects of their attitudes; the vertical axes represent their senses of control or lack of control over these objects. The attitudes are arranged circularly to illustrate the interrelationships between components more clearly. The attitudes listed in the inner circles are less extreme than those listed in the outer circles. Vertical relations between attitudes are symmetrical and horizontal relations are asymmetrical. The basic components of the attitudes listed do not change from one index

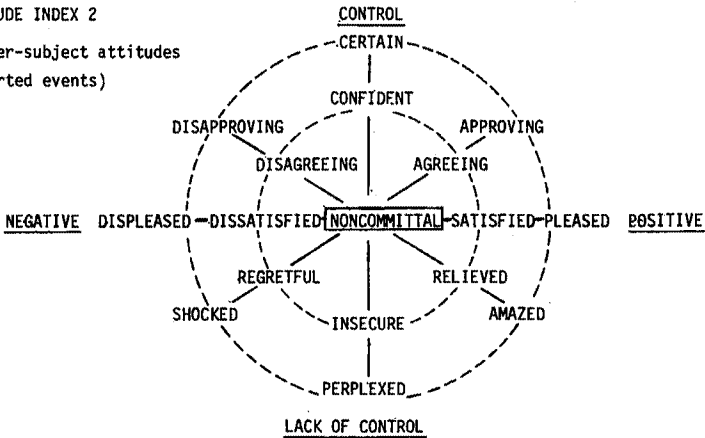
ATTITUDE INDEX 1

Speaker-listener attitudes



ATTITUDE INDEX 2

Speaker-subject attitudes
(reported events)



ATTITUDE INDEX 3

Speaker-subject attitudes
(projected events)

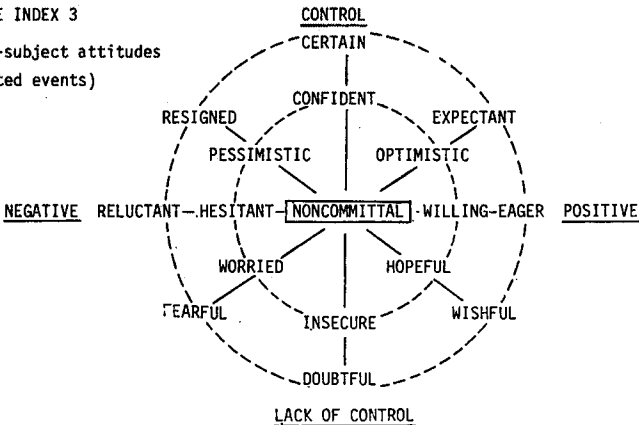


Figure 6. Attitude indices

COMMUNICATIVE GOALS: OFFERING ASSISTANCE												
COMMUNICATIVE OPTIONS												
VERBAL				KINESIC			INTERPRETIVE PROJECTIONS					
INTER-STYLISTIC CHOICES	INTRA-STYLISTIC CHOICES	TOKEN UTTERANCES	PITCH NUCLEUS PLACEMENT	PITCH DIRECTION	PITCH PROMINENCE	POSTURE	FACE	HEAD	EMOTIONAL	ATTITUDINAL		
Ego-near	1	John, I'm going to help you	help	↗	extreme	visibly tense	frowning	raised	strong	negative	self-assertive critical	displeased reluctant
	2	Need help, John	help	↗	pronounced	relaxed	smiling	level	weak	positive	neutral obliging	confident willing
	3	Want me to help, John	help	↗	normal	relaxed	smiling	level	medium	positive	neutral friendly	agreeing willing
Ego-neutral	1	Let me help you, Mr. Williams	help	↗	pronounced	relaxed	smiling	level	weak	negative	neutral reserved	unsure
	2	Do you need help, Mr. Williams	help	↗	normal	visibly tense	broadly smiling	lowered	medium	negative	neutral obliging	agreeing willing
	3	Should I help you, Mr. Williams	help	↗	extreme	relaxed	smiling	level	weak	positive	self-assertive aloof	displeased reluctant
Ego-distant	1	Sir, allow me to help you	help	↗	pronounced	visibly tense	smiling	level	strong	positive	self-assertive friendly	agreeing willing
	2	Do you require assistance, sir	help	↗	normal	relaxed	smiling	level	medium	negative	neutral reserved	unsure
	3	Would you like me to help you, sir	help	↗	normal	relaxed	smiling	level	weak	positive	neutral obliging	hesitant confident

Figure 7. An interactive exercise.

Figure 7. An integrative framework of communicative options and interpretive projections

to the next, but the terms relating them to their objects do, implying that shifting from one object to another requires a linguistic shift as well.

Interpretive projections are derived by analyzing patterns of verbal, prosodic and sociokinesic choice. A token utterance establishes a linguistic field of potential attitude objects. Pitch nucleus placement creates a point of semantic focus within this field. Pitch direction and head position signal volitional responses to this point. Pitch prominence and body posture signal the intensity of the emotional charge associated with it. Facial expression signals the direction of this emotional charge. In cases of conflict, prosodic signals override lexico-syntactic signals and sociokinesic signals override prosodic signals. The result is a highly simplified type of cross-modal interpretive process.

9. Conclusion

Throughout the paper we have tried to suggest that an important step in developing an adequate approach to studying linguistic interaction is to move away from the single-channel analysis of speech as language behavior and adopt a broader multichannel perspective from which speech can be analyzed, described and explained in relation to other forms of human communicative behavior. For the notion that the focus of linguistic inquiry must turn outward into the human context in which language occurs, we are, of course, greatly indebted to Professor Kenneth Pike. One way to reduce this context to a linguistically useful notion might be to integrate the procedures we have discussed here into a framework such as that in Figure 7.

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In the brief space remaining it is impossible to fully express the authors' indebtedness to all those scholars whose works have influenced the development of our interactional grammar. The following abridged bibliography is intended for the reader's general reference only. Those interested in more comprehensive references are invited to consult the bibliographies in Arndt/Janney 1979b, 1980, 1981, 1982, and forthcoming.

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THE PARAGRAPH: WHAT IS IT?
SOME TRADITIONAL AND CONTEMPORARY PROPOSALS
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In his opening address to the 'Focus on Discourse' colloquium in Toronto this year, Gleason spoke of the state of discourse study and the need for further research in specific areas: story structures, conversation and structures larger than the sentence. The question is, do we have the theories and descriptive tools to cope with such matters? In this paper I wish to address this issue in terms of work done on the paragraph. After looking at some traditional definitions of this unit, I will examine how certain linguists have dealt with units larger than the sentence within their theoretical frameworks. Finally, I will suggest some new categories of analysis in the light of which the concept of paragraph can be reassessed.

Webster's Third defines the paragraph as a distinct orthographic section of a printed composition that consists of one or more sentences and forms a rhetorical unit in that it deals with a particular aspect of the subject, or comprises the words of a distinct speaker. The Oxford English Dictionary's definition is much the same except it is not as explicit about the medium. In contemporary linguistic terms we might call the paragraph, according to these definitions, an orthographic and semantic unit. Semantically, these definitions are not very precise, yet, users of the language seem to make consistent paragraph distinctions according to some sort of semantic criteria.

Discourse centred tagmemists such as Pike and Longacre have dispelled some of this obscurity by their work in a number of different languages. In the light of these investigations, they have decided that the paragraph is a unit of the grammatical hierarchy, larger than the sentence and smaller than the monologue and discourse. The nucleus of the paragraph is filled by a sentence or sentence cluster as are the possible margins of amplification, illustration and so on. Pike, in his paired hierarchy sees the paragraph as the minimum unit of the expanded unit monologue, as clause is to sentence (Pike and Pike 1977). Whereas Longacre, rejecting the implications of these 'pairings' defines the paragraph according to his split level grammatical hierarchy. His paragraph is one of eight surface structures which has an isomorphic notional or deep structure called repatee (Longacre 1976: 165 ff). By identifying repatee as a notional correlate to paragraph, Longacre can make two kinds of statements. As well as coping with the notional characteristics of repatee, he likens the structure of the paragraph to that of discourse, with its aperture, introduction, pre-peak episodes, peak, post-

peak episodes and finis structures. The different sections within the paragraph are simply briefer and less clearly marked. It is of particular importance that in the work of both Pike and Longacre their theoretical statements are continually refined as their practical descriptive experience expands.

Grimes is not sure that paragraphs are the building blocks of discourse as Longacre suggests. However, he does view discourse in a similar fashion. Like other behaviouremes, or segmental chunks of behaviour, discourse is recognized as an entity with closure, where there is a beginning, an ending and an internal structure. Grime's distinction between semantic structure, the decisions a speaker makes regarding what to say, and the patterns available for implementing the results of those decisions, has some affinity to Longacre's notions of split levels as well. Although Grimes is hesitant to define the paragraph without recourse to the complexity of the particular text from which it is extracted he has developed a concept, the span, which is pertinent to paragraph analysis (see Grimes 1975: 109). Spans represent stretches of text within which there is some kind of uniformity. A text can be analyzed according to the types of spans and the units differentiated, as certain spans correspond to one another. Spans of time, place and participant orientation that interrelate frequently, correspond to recognizable surface structures larger than the sentence and can conveniently be called paragraphs (ibid.). Becker notes that English paragraphs are often divided according to participant spans. Within a paragraph a single participant maintains a higher profile of activity than others. Wise and Lowe note a similar phenomenon in Nomatsiguenga (noted in Grimes, op. cit.: 105).

Beekman and Callow have made a number of important advances in discourse studies, particularly in their delineation of semantic units, and their development of Grime's notion of span. Like both Longacre and Grimes, Beekman and Callow differentiate between semantic structure and surface structure, and like Grimes they too postulate that the semantic structure deserves special attention since it is somehow more basic and universal than a language's specific surface structures. In their 1981 The Semantic Structure of Written Communication, they outlined a hierarchy of four semantic units: component, concept, proposition and propositional configuration. Beekman and Callow wrote that 'every discourse consisting of more than one proposition is analyzable into a system of recursively intertwined propositional configurations and ultimately is reducible to a single propositional cluster' (1981:68). Their semantic paragraph, a distinct unit from our traditional paragraph, consists of one or more statements or propositional configurations and a single theme, derived from the main proposition of one of the included statements or the abstraction derived from several. It, like the episode and so on, distinguished in 1974 as semantic units, is now seen as appropriate for only certain genres or discourse types.

Perhaps even more important than their definition of this kind of paragraph, is their development of the span. The adoption of a tri-functional perspective on language was influential in this regard. Resembling Halliday's three metafunctions: the ideational, the interpersonal and the textual; Beekman and Callow distinguished three dimensions of meaning: the referential, the situational and the structural. When two or more spans carrying referential, situational or structural meaning coterminate, succeeded by new spans, propositional configurations can be delineated. Consequently, in units of discourse larger than the sentence, such as the paragraph, span cotermination can be a useful tool in determining both boundaries and internal structures.

Working within the systemic tradition towards the development of a 'communication' linguistics, Michael Gregory and I also view language as tri-functionally organized. Initially, we considered paragraph to be largely a semological matter. However, in the light of work done by Fleming (1974) and Larson (1978) in the stratified communication model, and Pike and Pike in tagmemics (1977), and our examination of the chains of functional choice in various types of discourses, we have reconsidered. We, like Sinclair and Coulthard (1975:23), have been troubled by the apparent ease with which many linguists have added units larger than the sentence to the hierarchy or rank scale containing those units smaller than the sentence. Edmiston, at last year's LACUS Forum gave a paper on the paragraph in Kosená. In this paper he examined the discourse distribution of the final verb form, to show that the paragraph was not just a semantic unit, but was a referential unit as well. Beekman and Callow call it both a semantic unit and a communication unit. To deal with such problems, Gregory and I have devised two separate, but related realizatory cycles to accommodate what we see as being two different types of units. Language as a complex code, we organize as most linguists do into three strata: the semological, the morphosyntactic and the phonological. However, for language as an activity, a behaviour, we have another cycle. We have a situation plane and a manifestational plane, and where the two interface we have a third, the plane of behaviour. It is on this plane that behaviour may be manifested more particularly as communicative behaviour and this, most particularly as discourse. It is on this plane that the unit of the paragraph is meaningful. The paragraph has a semological, a morphosyntactic and an orthographic structure; but it has more. The structure that Longacre likens to that of discourse, and that Beekman, Callow and Grimes also recognize, and that high school texts consider the 'norm', is a structure that is activated only in discourse. It has implications in the realizations of all three strata.

We did not arrive at this conclusion by examining the paragraph specifically, but by utilizing some tools we had developed earlier. Gregory first used the term phase in a paper analyzing one of Hamlet's soliloquies (1981 and forthcoming). In 'Generic

Situation and Discourse Phase: An Approach to the Analysis of Children's Talk', a study Gregory and I prepared last year, we continued to develop the concept of phase, and added to it that of transition.

Phase characterizes those stretches of text where there is a significant measure of consistency in what has been selected within the experiential, interpersonal and textual, or as we prefer, discourse functions. In a particular phase certain participant, process and locational chains may be featured, or one might say there have been particular transitivity choices made by the user of language morphosyntactically, semologically and phonologically for experiential reasons. Simultaneously, certain choices will have been made interpersonally in mood, modality and attitudinal selections. There will also have been certain theme-rheme, focus and prominence and cohesion patterns selected for discourse enabling purposes. So, three sets of meaning choices will have been made, and in a single phase these take on a particular pattern of interrelation. For example, in our children's spontaneous speech text, when first person participant chains predominated, along with declarative, future intentional modalizations, unthematized choices with exophoric reference only, we decided we had a single phase. This is not the same as Beekman and Callow's propositional configuration that span cotermination delineates. In cotermination, only the latter boundary, or the ending, is by necessity distinguished. The coterminating spans may begin independently. Spans overlap continuously and when two do terminate at the same point, the unit that is distinguished need not be 'phasal'. A participant span and event span may start independently and end together, while other spans that started with the event span may continue. This, then, would not constitute a phase and would not in all cases distinguish a propositional configuration. In a phase, all meaning choices interrelate in a particular way and this interrelationship establishes both initial and final boundaries.

When changes in one or more functions or meaning choices do take place and our phasal consistency is diminished, we have two possibilities. A new phase may begin immediately, creating new patterns of functional interrelationships, or the functional changes may be partial and gradual forming a brief bridge between the last phase and the new phase. Such a bridge we call a transition. In the language of some children we studied, there were many fewer transitions than we had supposed, one phase seemed to 'jump' right into the next (Gregory & Malcolm, forthcoming).

What is recognized as a phase will depend on the degree of delicacy, that is the depth of distinguishing detail of analysis. The delicacy of the description may be dependent on both the purpose for which it is being made and the size of the discourse under examination. A long discourse analyzed at not too great a degree of delicacy may be seen to consist of a small number of phases which constitute its overall pattern. Each of these phases may be divisible into further phases and transitions if the delicacy of the description of functional choices is increased.

It was in attempting 'phasal' analysis that we first realized we needed both a tri-stratal cycle to describe language as code and a tri-planal cycle to describe language as activity. Phase is not a semological unit as we first thought, but a series of semological and morphosyntactic choices. Unlike a paragraph, a phase need not have a beginning, middle and end structure, but the way the realizations of the metafunctions interrelate does mean that it is more than just a matter of the semological or morphosyntactic units which realize it. This means that we do not see phase as unit in a semological or morphosyntactic rank scale or hierarchy.

A simple experiment I conducted established the importance of the concept of phase to the internal workings of the paragraph. I asked a number of people to complete an exercise I found in J.A. Bright's Patterns and Skills in English. The exercise was to rearrange eleven graphological sentences into an orderly paragraph. Bright's purpose was to point out that it is the most general sentence that usually introduces a paragraph and names the topic, a point Pike and Pike would seem to agree with. In several high school texts this characteristic of the paragraph is noted. Some go further in outlining the internal character of this unit. They point out that subsequent sentences develop the topic and the final one concludes it with much the same information as the introductory sentence but with the addition of an attitudinal authorial comment (Ford 1960, Pollock 1955, Brewton 1966). All the people I asked to complete the exercise did begin the paragraph with the same sentence. However, after that there was variation in their response, but variation that was patterned.

To try and understand the variation in task completion, I analyzed the paragraph tri-functionally: morphosyntactically and semologically. Within the paragraph I identified two major phases where functional selections made consistent patterns of interrelation, and the beginning of a third in the two clauses of the final sentence. The first five clauses realizing the first phase were characterized by various types of relational processes, declarative mood selections, a lack of thematization, particular conceptual and collocational lexical chains and a noticeable lack of inter-sentential cohesion. In the following six clauses, realizing the second phase, declarative mood and unthematized selections had been made as well, but action and mental processes were now featured along with relational processes. In addition, embedding began, passivization and inter-sentential 'linking'. Generalized lexical items were replaced by specific ones dealing with elephants and lions. The final three clauses were declarative also, but were thematized by adjuncts of place and time. The links and action processes of phase two disappeared, and new lexical chains emerged. Hence, although this last section of the paragraph was too small to exhibit much consistency, one would have to say that it looked like a distinct third phase. This phasal analysis of the para-

graph showed that the variation in its organization by different people was patterned according to its phases. After the introductory sentence some participants in the experiment had proceeded with the first phase, others with the second. Sentences were not isolated randomly, but were kept intact within the particular phase and it was the phase as a whole that was open to repositioning so the variation would seem to be discourse organizational. The final phase was recognized as such by most, but probably because of its limited size, some tried to combine it with phase two, which it more closely resembled.

With a more delicate analysis phase two could be divided into two smaller units, one following the 'elephant' lexical chain, the other the 'lion' chain. The morphosyntactic distinctions between the two were so slight, a matter of embedding and linkage, and the two units so small that I did not feel justified in calling them two distinct phases. The interrelationships in functional selections didn't have the 'space' to become consistent, before they changed. It has been mentioned that the number of phases distinguished in a paragraph are dependent on the perspective of the analyst. The paragraph could have been part of a larger single phase featuring declarative mood selections as opposed to interrogative selections, transitivity chains involving animals as participants as opposed to humans and so on. From my perspective three phases are discernable with the most complex of the three involving another possible sub-division. In addition to these three phases there are two transitions, one before phase two and one following it, as shown in the appendix. In both cases some, but not all the functional selections have changed. The transitional sentence echoes certain selections in the previous phase and at the same time anticipates changes that are going to be made in the following phase.

Recognizing that traditional definitions of paragraph are too vague to be of analytical use, this paper first examined suggestions of selected linguists as regards viable concepts of paragraph in relation to other stretches of discourse. Traditional definitions of the paragraph were based on orthographic conventions for which semantic justifications could then be made. Contemporary linguists, turning their attention to the analysis of discourse, used the term for stretches of text larger than the syntactic sentence in spoken texts as well as written. In doing so, they focussed on the semantic component of the original definition and ignored the orthographic and medium restrictions. Consequently, the nature of the paragraph was inadvertently changed and changed without explicit recognition. To add to the confusion, regarding the handling by contemporary linguists of the semantic component of the traditional definition, contemporary theories of language are essentially theories of the code, and paragraph sits uneasily within codal statements. The paragraph has been called an orthographic unit, a morphosyntactic unit and a semological unit. It has also been used as a communication unit and a referential unit in theories which recognize the importance of language as an activity in

communication situation, referential realm, etcetera. However, these theories include such considerations as an extra stratum within a single realizatory cycle which is code dominated. In other words, models which are essentially sentence productive are being used in the clearly interpretive task of discourse analysis (cf Gleason 1980).

Gregory and I see discourse as a realization of the code in communicative behaviour which is situationally manifested. I suggest that the tri-functionally characterized categories of phase and transition, which are open to being used with varying delicacy, can throw light on the particular organizations of particular discourses, the paragraph for one. Confusion surrounds such units, particularly those which have been re-defined by contemporary linguists. As Gleason said, larger than the sentence discourse units need further research. But, before we do this research, or research on story structures or conversation, we need theories and descriptive tools which do not impose code restrictions on activity.

Appendix

(taken from J.A. Bright's Patterns and Skills in English, Book 1, p.60)

Phase 1

1. East Africa is a wonderful place for animals, tame and wild.
2. The long-horned Ankok cattle are very handsome beasts.
3. A large bull may have horns eight feet apart at the tips.
4. It is, however, the wild animals that are famous.

Transition 1

5. In the numerous game parks there are huge herds of elephants.

Phase 2A and B

6. A visitor is most unlikely to come away without having seen elephants doing something interesting.
7. We have seen them taking dust-baths, washing youngsters and pretending to be submarines with only their trunks above water.
8. Lions are less plentiful than elephants
9. and more difficult to find.
10. We have, however, seen the lions in the Nairobi Park several times
11. and taken some good pictures.

Transition 2

12. Once we found the famous tree lions at Ish. in the Queen Elizabeth National Park.

Phase 3

13. Even outside the parks it is not uncommon to see baboons
on monkeys,
14. and once by the side of the road I saw a chimpanzee.

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FORM VS. FUNCTION IN DISCOURSE STUDIES

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Linguists have always had trouble distinguishing themselves from grammarians. While the latter group has been a part of human civilization for centuries, it has been only in the present century that people have deigned to think of themselves as linguists. To do so, a number of oppositions such as description vs. prescription and norm vs. rule had to be invented. With time we linguists even recognized indeterminacies in language structure and variations dependent on social markings. We showed our worth to the world by investigating how children learn to speak and how human language differs from animal communication. We did many things to prove that we were different from grammarians but in all that we did, we never abandoned grammar. Instead, we built upon it and the resulting edifice is an imposing one, indeed.

Unfortunately, this grammar-orientation of ours may have seriously limited the way we will look at discourse. We seem to assume that there will always be a grammar which underlies and motivates what people say. This grammar has an existence in and of itself in the minds of many linguists. When utterances do not match the expectations of what grammar should produce, we interpret them as deviant or lacking in cohesion. Dore and McDermott (1982) provide an example of what we are likely to find in the most contemporary writings of linguists as they approach discourse. They describe the verbal behavior of children in a classroom as follows:

As might be expected, Rosa's I could read is the most interesting and complex utterance interactionally...apart from its propositional ambiguity and functional equivocality, it was interactionally indeterminate. Here we give the interactional evidence for its being a PRAGMATIC COUNTERFACTUAL, i.e., an utterance that is treated as a reversal of what it literally expresses.

Why must analysts be so encumbered with burdensome theories of what people do with language form that they

are led to invent clumsy terminology to label what does not fit? A 'pragmatic counterfactual' exists only for those who assume that people begin with expressing themselves literally. If we assume that utterances have a literal base then we run into trouble with idioms which must always be counterfactual. Perhaps language is layered or stratified in ways to account for all mappings of literal/non-literal senses even in discourse. However, I hold out hope that much of this heavy theoretical gear can be avoided if we confront discourse on its own grounds.

It is my premise that discourse is patterned but not rule-governed. While grammar may indeed be governed by rules which its users somehow know or have locked in their heads in some neurophysiological way, discourse seems to come about in quite different ways. Henry Widdowson's distinction between 'rule-governed' and 'rule-referenced' may be useful here. In Widdowson's view, grammar is rule-governed behavior while the use of language in discourse is rule-referenced. The latter term means that persons engaged in conversation are free to draw upon a set of conventions to form their discourse without being fully controlled by such conventions. For this reason, each conversation is unique yet intelligible--at least in theory--to those participating in it. For example, rules exist for taking turns in speaking but they are not necessarily obeyed by persons sharing the same conversation. Tannen (1981) has argued convincingly that overlap (i.e., two or more persons talking at the same time) can perform several diverse functions. In addition to taking issue with the speaker, the overlapper may be reinforcing the same viewpoint.

A number of different linguistic devices exist for changing the topic of a discourse. In English, we have many, ranging from a direct request to change the subject or a declaration that we are not changing the subject to no verbal request at all. Maintaining one's turn to speak even when one cannot find just the right words is also signalled in a number of ways. In English, we repeat a mid-central unrounded vowel (written: uh...uh...uh). Speakers of other languages use different devices, as in Latin American Spanish where the word *este* is repeated, or in Japanese, where the hesitation formula is manifested as the word *ano*. To counter an overlap which signifies disagreement, the speaker may simply raise his or her voice, as if to 'drown out' the overlapper. Yet another feature of conversational management is a form of editing which is called 'repair.' This correcting procedure may be done by the speaker of his or her own discourse or by

another party.

I offer features such as topic-change, overlap, hesitation formulas, and self/other repair as evidence that conversational discourse is shaped and directed by conventions which have little to do, if anything at all, with grammatical rules. Conversational speech appears to be quite different in nature from the sentence generation of individuals. As a consequence, the competence required to engage in conversation with others is quite diverse from the grammatical competence which has motivated much of our linguistics to date.

The differences between grammatical and conversational competence are never so apparent as in the context of learning another language. It is commonplace to hear students complain that after three or four years of studying French or Spanish they cannot talk with anybody. What, indeed, have they learned? A look at the kinds of tests used to evaluate their achievement is revealing: discrete points of morphology, syntax and vocabulary. Discourse and the functional uses of language are too difficult to test according to the means presently at our disposal and so these matters are ignored or glossed over in the assessment of student achievement. Textbooks traditionally present foreign languages as grammars, usually without any guidance from a unified theory of language. As an apology for what is done, the term 'pedagogical grammar' is introduced. A 'pedagogical grammar' is supposed to differ from a linguistic one in that it takes into account the ways points of grammar must be taught. While it can be argued that the differences between linguistic and pedagogical grammars must be trivial in a theoretical sense (two grammars of the same language must explain the same data, otherwise one of them is wrong), the more significant point is that the equation Language = Grammar holds for language teaching as much as it does for linguistics.

Prevalent in the thinking of foreign-language methodologists is the transfer theory of learning. According to this theory, students transfer what they have learned about a language in the pedagogical modes of contrived dialogs and structured drills to the natural mode of discourse. Any teacher with a few years of experience in the classroom soon learns how false this traditional theory is. What students learn in paradigms they repeat in paradigms. What they learn as clichéd dialogs they tend to repeat as such. It is doubtful that the spontaneity of discourse in a foreign language can be acquired by exercises which are, themselves, devoid of spontaneity.

In discussing the various types of foreign language performance in the classroom, Stephen Krashen (1981) uses the term 'prefabricated pattern' to characterize the type of language which is often found in audiolingual exercises. Patterned speech samples may come as isolated sentences with a slot to fill, e.g., 'That's a _____' (fill in: pen, book, letter, etc.) or even as dialogs which have the appearance of discourse, e.g.,

- A: What's that?
B: That's a pen.
A: Where did you find it?
B: I found it at the bus stop.
A: When did you find it?
B: I found it on Tuesday.

There are actually two issues here. The first begs the question of how valuable such exercises are in the first place. What transferability to real uses of language can we expect from prefabricated patterns and routines? The second issue relates to the characterization of discourse conveyed by such exercises. As illustrated by the sample 'dialog' given above, the pedagogical intention in audiolingual conversational routines is to focus on grammatical points. Matters of conversational management are addressed rarely, if ever. Claire Kramsch (1981) has tried to rectify this lack in her teaching approach by addressing the major participant skills in conversational management, i.e., taking turns, keeping the floor, buying time (hesitating), mitigation, interruption, framing, redirection, and so on. Kramsch provides both the type of vocabulary item and the phrases associated with each conversational skill. She also gives some sample classroom exercises which focus on the use of diverse strategies in conversation.

While Kramsch's exercises are far more sophisticated than the traditional kind of classroom drills, they do not bring the individual student to a commitment to use the language in real speech. The major concern in discourse remains with form and not the needs of speakers oriented toward personal goals. Kramsch does admit that students who are naturally shy and not very verbal may be left out of much classroom activity. Inevitably, the question is asked over and over again by the student of a foreign language: what do I expect to gain by learning this language? If my answer is a philological one, then I can be content with declensions, parts of speech, syntactic patterns, and lists of idioms. If, however, I expect to be able to communicate with speakers of the language then my focus on grammar must be changed. Unless I am a linguist and therefore a member of an unusual group of language learners, grammar becomes, for me, not an end

in itself but a means to an end. The same criticism applies to disembodied lists of rhetorical devices.

In all our efforts to teach students how to converse in a new language, we have ignored a simple but not so obvious activity, namely to bring the communicative needs of the learner to the language. Rather than saturate students with a vast array of grammar, vocabulary and rhetorical devices, we can try to place them in a position where they will take what they need from a pool of grammar and pragmatics in order to create a part in a meaningful discourse. Such an approach is being tried at the University of Delaware with the label 'strategic interaction.' The key element in this approach is the scenario. To resolve the issues presented to them in the scenario, the students act together as a group. They are allowed a 'rehearsal' period, during which they can request all the help they wish from the teacher. The scenario, itself, always allows for several participants to have differing intentions. For example:

Sample scenario. Student A is put in the role of a traveler who must return home after an important business flight to Tokyo. He/she arrives at the check-in desk at the airport and is told that the flight has been overbooked. The clerk offers a free additional night in a luxury hotel in Tokyo, with the promise of a seat on the flight leaving the next day. Student B plays the part of the clerk who has inadvertently overbooked the flight. His superior has told him that he must convince 10% of the passengers to give up their seats or he will lose his job.

Neither student is told of the other's intentions or desires. During the rehearsal stage, separate groups are charged with helping each student to prepare a number of things to say in anticipation of what the other will say. The performance stage follows, with each student listening keenly to what the other is saying. In this way, a more or less 'natural' situation is exploited to stimulate students to create discourse in the foreign language. To play their roles effectively, the students must decide what they would actually do under similar circumstances. Would an extra night in Tokyo be good? What about the business meeting I must attend tomorrow in New York? And so on.

We have begun to conduct research on how scenarios affect student discourse. In a paper to be presented at this conference, Lantolf and Khanji (1982) report

on how student discourse is affected by different sets of role relationships. Their research shows that discourses on diverse topics and between differently perceived relationships display changes in levels of accuracy in morphology, in length of utterance, and in syntactical phenomena such as embedding and use of metaphorical language. To offer another bit of evidence, this time in an anecdotal vein, students who have experienced instruction through strategic interaction tend to use their newly-gained competence outside the classroom in conversing with other students. They often report that they think of new things they could have said in the scenario after leaving class.

In my view, the concerns of the linguist doing research on discourse are the same ones around which the foreign-language teacher must build a sound instructional program. We seem to have taken a 'corpus' oriented approach to discourse. We collect specimens of conversation which we then analyze according to one of several ethnographic models available (e.g., Hymes 1972 or Schegloff and Sacks 1973). The corpus approach yields valuable data, to be sure, but the results are not tied together in a coherent and comprehensive theory of what motivates people to speak to each other. The alternative to a corpus-oriented approach is a functional one, inspired, perhaps, more by Halliday 1975, than by anyone else. In my particular interpretation, the term 'function' covers a range of communication behaviors which are best studied as 'personal competences.' At present, we can talk about four of these competences, as follows:

(1) Conversational management. Superficially, at least, conversations are cooperative affairs. Each participant makes use of a number of conventions which are established by the speech community in order to maintain the discourse. These conventions enable the conversing subjects to initiate speech, continue it and eventually bring it to a close. In addition to these so-called 'framing devices,' there are mechanisms for turn-taking, holding the floor and repairing mis-cues in speech (see earlier discussion, above). I have just said that conversations are superficially cooperative. We should remember that they can also be competitive, in which the participants do not follow the conventions dictated by society. Interruptions, overlaps, and abrupt terminations are features of a vast array of dialogs, from the exchanges that happen between drivers of cars involved in accidents to the heated bargaining of marketplaces in many parts of the world.

(2) Verbal tactics. Whereas conversational management skills are socially determined, the knowledge of verbal tactics is quite individual. Verbal tactics are used to advance specific points, counter opposing arguments, agree with others, and all the other matters which we traditionally group under the title of 'rhetoric.' Not all speakers are equally proficient in verbal tactics. Even the style of tactical language varies from person to person. For example, there are innumerable ways to give compliments. This state of affairs contributes to a kind of transactional ambiguity which goes beyond the structural type associated with grammar. Deciding whether a remark was a genuine compliment or a veiled insult depends on the hearer's interpretation.

(3) Verbal strategies. By 'strategies' I mean the ability to combine diverse tactics into 'game plans.' As we attempted to capture in the scenario illustrated above, people often 'think out' what they hope to gain before entering into discourse with others. Of course, there is no guarantee that all parties in a conversation will employ dovetailing strategies, each participant must be prepared for yet another kind of ambiguity, this time, one which sometimes results in one party saying, 'I wonder what his game really is...'.

(4) Role enactment. We all play a number of roles both verbally and nonverbally. These roles derive from participation in long-standing relationships such as 'parent/child' or 'friend/friend', as well as from short-term encounters such as those between a vendor and a buyer, a passenger and a flight attendant, or a policeman and a traffic violator. The language used to play diverse roles reflects age differences (e.g., 'I can talk to you that way because I am your mother'), disequilibrium in the power continuum (e.g., 'But officer, I was only going 35 mph'), and many other factors relating to the presentation of a 'public face.' Role interaction brings its particular kind of ambiguity. Even when we, as the other participants in conversations, recognize the particular tactic and guess at the transactional game being played, we still must decide on the role being played by our partner in discourse. Our interpretations are sometimes right and sometimes wrong, as soap-operas capture so well on TV.

To conclude, let me repeat my belief that the ideal approach to discourse is 'user-centered.' If we take such an approach, we can ask a new generation of questions: How do people recognize tactics and strategies? What is there in a given utterance that signals a specific interactional role? What elements of

language contribute to the formation of long-term scripts such as those played out between intimates? These and similar questions are the exciting ones that await us as we take on a human-centered study of discourse.

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LE POSTULAT LEXICAL: UNITE MINIMALE FONC-
TIONNELLE D'ANALYSE SEMANTIQUE DES TEXTES

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C'est dans le cadre d'une démarche analytique en vue d'expliquer les processus de liaison entre énoncés d'un texte produit correctement que nous avons dû redéfinir certains concepts théoriques relativement à la description sémantique des langages naturels.

Le texte analysé, contenant approximativement cinq cents mots, est un éditorial à saveur panégyrique écrit par l'écrivain romancier et journaliste francophone Roger Lemelin. Ce texte nous a semblé satisfaire tous les critères de sélection de deux propositions (P); les P sont sémantiquement interdépendantes; son contenu est cohérent. La compréhension du texte par un ensemble diversifié de lecteurs fait foi, a priori, de toutes ces caractéristiques. De plus, le texte semble contenir un grand nombre d'énoncés sous-entendus, lesquels constituent au premier regard un important réseau de P autour desquelles s'organisent, entre autres, la cohérence du texte.

Dès le départ, il est impératif d'établir des critères de constitution des unités minimales de prédication. Dans cette perspective, faut-il traiter les P sous-entendues comme des sous-ensembles des autres P de surface? Ou faut-il plutôt les isoler des P de surface pour les analyser indépendamment et de manière identique à ces P de surface? De toute évidence, les critères de constitution doivent être suffisamment extensifs pour être capables de représenter adéquatement la plus petite unité de sens sous-entendu.

Puis, dans un deuxième temps, on doit procéder à la différenciation des divers types d'énoncés sous-entendus, chacun d'eux étant susceptible d'intervenir différemment dans le processus de transformation sémantique. Tantôt, la plus ou moins grande subjectivité déterminera la catégorie du sous-entendu. Tantôt, la nature même de l'antécédent ou du conséquent de l'inférence constituera le critère de classification. Tantôt, les inférences seront ordonnées suivant la nature de l'opération logique: syllogisme, implication, présupposition, condition, inclusion, etc. Assurément, pour les besoins de notre analyse, il faut distinguer l'inférence logique de l'inférence pragma-

tique ou de l'inférence sémantico-linguistique. L'inférence dérivée du système sémantique linguistique tel que régi par un ensemble fermé de règles d'interprétation uniquement lexicales (mais jamais pragmatiques) est certainement la catégorie d'inférence la plus objective. Par objective, nous entendons "dénudée de tout jugement de valeur, de toute interprétation idiosyncratique". Mais encore, quelles en sont les règles de dérivation de sens? Et quelles en seront les règles de constitution?

Certainement des définitions de lexèmes telles que celles qui figurent dans les dictionnaires usuels de la langue française ne peuvent représenter adéquatement et totalement l'ensemble de toutes les inférences véhiculées par un lexème de surface du texte. Ainsi, pour

- (1) il fallait qu'il nous assène soudainement sa décision de partir,

les dictionnaires de la langue française définissent assener de la façon suivante:

G.L.L.F.: porter violemment un coup bien dirigé;

T.L.F.: assener un outrage, un verdict, des injures à quelqu'un; assener un avertissement à quelqu'un.

De toute évidence, de telles définitions sont incomplètes, obscures, souvent circulaires et beaucoup trop extensives. Elles ne peuvent expliciter et isoler chacune des unités de sens véhiculées par le lexème assener. C'est alors qu'une analyse plus détaillée et une formulation du définiens en unités minimales de sens du type

assener ———— > (P₁) x exprime quelque chose à y;
 (P₂) x exprime soit une parole, soit un discours, soit toute autre forme d'argumentation;
 (P₃) la manière dont s'exprime x est violente;
 (P₄) la manière dont s'exprime x est surprenante

permet d'énoncer quatre P sous-entendues et inférées du même lexème. P₁ à P₄ constituent ce que nous appellerons plus tard les postulats lexicaux (PL) d'un lexème.

En 1951 Quine, puis en 1952, Rudolf Carnap introduisent le concept de postulat de sens (meaning postulate). Carnap restreint toutefois ce concept à l'analyse des langages formels: "Il m'apparaît que l'explication de concepts de ce genre pour

les langues naturelles génère des problèmes de nature tout autrement différente". Il définit le postulat de sens (PS) comme suit: règle sémantique capable d'expliquer le concept d'analyse, c.à.d. le concept de vérité basé sur le sens, à l'intérieur d'un système sémantique.

PS: (x) (B x $\supset$ M x)

Toutefois, Carnap ne voit pas la nécessité de distinguer interprétation sémantico-logique et interprétation sémantico-linguistique.

Si on veut appliquer une telle règle pour dériver les P sous-entendues d'un texte en langue naturelle, il faut nécessairement en modifier les conditions initiales de dérivation. D'une part, l'opération logique qui relie l'antécédent au conséquent est à la fois inclusive et implicative (si P_2 alors P_1). D'autre part, la règle exige que le conséquent ne contienne aucun item lexical contenu dans le definiendum, et finalement, le conséquent ne contient pas nécessairement une négation de l'antécédent. De manière à éviter toute confusion possible avec le PS de Carnap, nous appellerons cette nouvelle règle le postulat lexical:

PL: (x) (B x $\rightarrow$ $\neg$ M x)

Ainsi, en appliquant cette nouvelle règle à l'analyse de l'énoncé suivant:

(2) Pierre Trudeau est un homme vulnérable,

on obtiendra le PL suivant:

PL₁: (PT) (vulnérable (PT) $\rightarrow$ pouvoir facilement être dominé psychologiquement par Y (PT))

Une règle d'interprétation de ce genre peut générer autant de definiens (P sous-entendues) que nécessaires pour une analyse du lexème. Elle peut expliciter et atomiser chacun des sèmes élémentaires véhiculés par un lexème.

Chaque conséquent du PL est formulé a) à partir des informations lexicales fournies par les dictionnaires de la langue, et b) à partir des définitions spontanées fournies par un échantillon d'informateurs dont la langue maternelle est identique à celle du texte étudié.

Relativement à l'analyse de texte, la règle du PL garantit une interprétation plus objective de ses sens. L'ensemble des règles auxquelles se réfère le locuteur pour dériver les défini-

tions de lexème, soit le lexique de la langue institutionnalisé dans le dictionnaire et dans ses locuteurs de langue maternelle, est plus directif et plus délimitatif que tout autre univers de données. Par ailleurs, la règle du PL peut décrire adéquatement ce type de sous-entendus en unités de prédication extrêmement élémentaires.

Relativement aux opérations de liaison des unités à l'intérieur d'un texte, la règle du PL rend plus claire l'analyse de ces liaisons de par sa capacité à mettre à jour tout un ensemble d'unités sur lesquelles opèrent ces processus de liaison. Ainsi, autant l'unité sur laquelle s'opère la liaison peut être sous-entendue, autant la liaison qui relie deux P à l'intérieur d'un texte peut être implicite. Beaucoup plus rares sont les relations de cohérence explicites.

Pour ne relever que les mécanismes de liaison les plus évidents et fort possiblement les plus déterministes, nous citerons très brièvement ceux observés en cours d'analyse. Puis on s'attardera un peu plus longuement à discuter le principe de cohérence en tant que mécanisme de liaison.

- le principe de restriction qui, par mode de sélection, permet de relier les items lexicaux sur l'axe syntagmatique;
- le principe de combinaison sémantique qui permet de relier les items lexicaux d'un syntagme suivant la compatibilité des traits sémantiques de chacun des items;
- le principe de redondance qui permet de relier deux P consécutives sur la base d'une unité posée dans la première P et répétée dans la seconde;
- le principe de progression qui permet de relier deux P consécutives pourvu qu'un nouvel élément de sens soit contenu dans la deuxième P;
- le principe de cohésion, différent du principe de cohérence, rejette la présence simultanée de deux P incompatibles à l'intérieur d'un même texte. Ainsi, 'si x est bleu, alors x n'est pas mauve'. Le critère de compatibilité logique est appliqué en vertu des classes d'unités pragmatiques ou des paires d'unités logiques. La cohésion est déterminée non pas par les règles du lexique de la langue mais plutôt par les règles du sens commun et par les règles du raisonnement logique. Dans ce sens, le PS de Carnap peut permettre d'analyser la cohésion des textes.

Le principe de cohérence diffère du principe de cohésion par sa grammaire d'interprétation, celle-ci lexico-sémantique du

I. Bellert et P Weingartner (1979) ont formulé une définition opérationnelle de la cohérence sémantique:

Pour tout énoncé d'un texte, il faut que cet énoncé (seul ou avec ses hypothèses et son ensemble de référents (relevant background) ait soit (au moins) une conséquence en commun avec (au moins) un de ces prédécesseurs ou soit que cet énoncé ait un ensemble de prémisses en commun avec au moins un ensemble de prémisses appartenant à ses prédécesseurs.

Suivant cette définition, l'ensemble de référents (relevant background) réfère à une vaste catégorie de sous-entendus contenus dans un texte. Cet ensemble est lui-même sous-ensemble d'un ensemble plus vaste contenant des jugements de valeur, règles, normes, faits connus, suppositions, croyances, etc. Les référents logiques tels que constitués à partir des lois de la logique et des règles d'inférence logique sont éléments de l'ensemble de référents. Bellert-Weingartner ne semblent pas voir la pertinence d'isoler le postulat lexical de l'ensemble des autres référents: [..] there are definitions or "analytical" statements or meaning postulates [?] postulats lexicaux in the background which connect a predicate occurring in the text ". Certainement, cette dernière affirmation est tout à fait juste pour analyser la phrase suivante:

(3) homme timide, sensible et vulnérable dans les dialogues à deux, il (Pierre Trudeau) devient tout autre lorsque le cercle s'élargit à plusieurs personnes.

Ainsi, (3) contient P_1 'PT est un homme vulnérable' et P_2 'PT devient tout autre'. En appliquant la règle du PS au lexème définisseur de l'antécédent vulnérable, on obtient la P inférée 'PT peut facilement être dominé psychologiquement par Y'. De même pour tout autre on obtient la P inférée 'PT est supérieurement différent de P énoncé précédemment'. De ces deux P inférées on peut poursuivre le raisonnement en concluant une troisième P inférée 'PT est fort', d'où le lien entre P_1 et P_2 . Assurément, tel qu'énoncé dans la définition précédente, le PS de vulnérable, élément indistinct de l'ensemble des divers référents, relie sémantiquement et de manière cohérente P_1 à P_2 .

Toutefois, suite à l'application de la définition Bellert-Weingartner à notre texte, nous avons cru déceler l'existence de degrés variables de cohérence suivant la position du PS à l'intérieur de l'opération logique. Il est tout à fait

raisonnable de croire que plus une P sous-entendue s'éloigne du lexème initial du texte plus il y a risque que cette P soit affectée de subjectivité et de valeurs pragmatiques. Puisque le critère d'objectivité (absence de jugement de valeur, d'interprétation pragmatique) est de toute première importance pour l'interprétation sémantique des textes, il devient alors justifié d'exiger une distinction entre P inférées directement d'un lexème du texte et les autres P inférées. Considérant à nouveau l'exemple (3), conviendra-t-il encore, à la lumière de cette dernière clarification au sujet du critère d'objectivité des inférences, d'analyser la troisième P inférée 'PT est fort' de la même façon que les deux premières? Convient-il d'inclure les deux premières P inférées dans le même sous-ensemble de référents que pour la troisième, bien qu'elles diffèrent à certains égards. En effet, alors que VULNERABLE_{ps} et TOUT AUTRE_{ps} sont des enchâssements d'inférence de premier niveau, c.à.d. qu'ils sont dérivés directement d'un lexème du texte de la façon suivante:

vulnérable —————> PT peut facilement être dominé psychologiquement par Y;

tout autre —————> PT est supérieurement différent de P énoncé précédemment,

la troisième P inférée n'est pas dérivée directement d'un lexème:

PT peut facilement être dominé
psychologiquement par Y

ET

PT est supérieurement différent
de P énoncé précédemment

—————> PT est fort

'PT est fort' est, en fait, la conclusion d'un raisonnement à deux prémisses, celles-ci étant les PS des lexèmes du texte. Un tel raisonnement utilisant les PS de lexème comme données de base est appelé enchâssement d'inférence au deuxième niveau. Certes, sans ce cas-ci, l'inférence 'PT est fort' est conforme aux règles d'équivalence du système lexico-sémantique français. Heureusement, le principe d'objectivité se trouve ici respecté bien que l'enchâssement soit de second niveau. Toutefois, dans certains autres cas, la conclusion du raisonnement d'un raisonnement semblable peut être dérivée de règles d'équivalences autres que lexico-sémantiques telles que système de croyance, système pragmatique, etc. Tel serait le cas si le raisonnement avait été du type:

PT peut facilement être dominé
psychologiquement par Y

ET

PT est supérieurement différent
de P énoncé précédemment —————→ PT est dominant

Dans ce cas-ci, la conclusion est beaucoup plus un jugement de valeur inféré des PS de vulnérable et de tout autre. En effet, en aucun lieu, le lexique français ne stipule que 'être dominant' est sémantiquement équivalent de 'être supérieurement différent de être facilement dominé psychologiquement par Y'. Nécessairement, la présente déduction y perd en objectivité et en précision quant aux liens dérivationnels.

Cette dernière démonstration prouve à elle seule la pertinence du postulat lexical comme unité d'enchaînement de premier niveau, distinct du postulat de sens de Bellert-Weingartner et même distinct du postulat de sens de Carnap. De là la nécessité de distinguer postulat lexical et postulat de sens ou toute autre catégorie d'inférence textuelle. Le postulat lexical, par sa définition, telle que formulée ici, permet de garantir une plus grande objectivité à l'analyse d'énoncés sous-entendus d'un texte tout en constituant une unité fonctionnelle de liaison entre les P de surface ou les autres catégories de P inférées du texte.

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LINGUISTIC SIGNALS OF ACTIVENESS AND PASSIVENESS IN
LAWRENCE DURRELL'S THE ALEXANDRIA QUARTET

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In the 1950's Lawrence Durrell began writing a series of novels about Alexandria, Egypt. The purpose of writing his story as a group of novels rather than a single book was to illustrate his view of life as a complex interrelationship of self and the world around that self. What emerged from this ambitious project was a quartet of novels, each book of which views the same events or story from a different perspective.

One way in which Durrell presents different aspects of reality is by varying the degree to which the characters in the different novels have control over circumstances and the environment. This control over the situation is expressed by a feeling of activeness or passiveness in each book. The terms activeness and passiveness are being used here not necessarily in the technical or grammatical sense of active/passive voice but to express the feeling conveyed to the reader of the sense of active control over the situation. Does the character make things happen or do things happen to him? While this degree of activeness is an integral part of each novel involving many characters and situations the feeling is most obviously conveyed through the main character. In three of the four novels, the central figure is the first person narrator or the 'I'. In the other novel (the third in the series) the main character is presented in the third person.

In the first novel, Justine, the narrator, Darley, seems to be in charge of his destiny. He controls events and is confident of his interpretation of reality. In the second novel, Balthazar, both Darley and the book as a whole still exude a feeling of active participation and assuredness, but the total confidence of Justine begins to waver. In contrast to Justine and Balthazar, the third novel, Mountolive, presents its main character, Mountolive, as a victim of circumstance who floats with the tide and is influenced rather than influencing. Finally in the fourth novel, Clea, Darley also feels himself a victim of circumstance and realizes it was not he who "shaped reality" but that reality shaped him.

The Alexandria Quartet is very form-conscious. This is not a subtle or unconscious working of language but rather a bold and deliberate manipulation of linguistic structures to achieve a desired effect. There are a number of linguistic devices that Durrell uses to convey the degree of active participation in each novel. An examination of the verb processes and relationship of the major participants to these processes is particularly revealing. I have used M.A.K. Halliday's model of grammar in general

and his development of the concept of the clause as a process in particular, as a working structure in the analysis of Durrell's work. The beginning approximately 800 words of each book have been selected for analysis. The beginnings were chosen because they are most likely to be structurally important to the books, and because Durrell, like most authors, uses the opening passage to set the tone and establish a mood for the reader.

The analysis was carried out in basically three stages. First, the entire beginning passage of each book was analyzed. Each main verb was identified as one of Halliday's four principle types of process (Halliday: 1977): material (Ma), mental (Me), verbal (V), or relational (R). (See Chart I) Then each participant function in that process was labelled (e.g. actor, phenomenon, attribute, sayers, etc.). For the purposes of this research, I have further defined actor, cognizant, and sayers as active participants (AP), and goal, beneficiary, and even circumstantial element as passive participants (PP). Finally each participant was identified as functioning as grammatical subject of the clause or grammatical complement (that which functions in traditional grammar as object or complement).

Second, all material processes were charted. Material processes were analyzed in detail because the material processes are where the action occurs and we are concerned here with activeness versus passiveness. These included not just the main processes of step one but any material process. The actor (A) of each process was identified (or marked missing) and labelled as grammatical subject (S) or grammatical object (O). (See Chart II) The goal (G) and beneficiary (B), if any, were likewise identified and labelled. All the actors were then categorized into lexical sets. Six major lexical sets surfaced: 1) the main character, either 'I' or Mountolive (M); 2) other humans; 3) natural phenomena (e.g. wind, the sea); 4) inanimate objects (usually the boat or a letter); 5) place (usually Alexandria); and 6) a more nebulous sixth category that includes such phenomena as knowledge, time and space, and reality.

Third, all processes involving the main character (the 'I' or Mountolive) were analyzed. The main character was identified as subject or object and then labelled as to participant function or in a few cases as a circumstantial element. (See Chart III)

The first novel in the *Quartet*, *Justine*, gives out a definite feeling of assuredness and active control of life. The main character, the narrator, feels he is in control of the situation. We, as readers, feel not only that Darley has control but that life in general is something we can direct. This feeling of activeness and control is born out linguistically, especially when *Justine* is compared to the other three novels. Indeed, the beginning passage of *Justine* is written entirely in the active voice with only two possible exceptions: 1) the grammatically passive but perhaps more logically relational process of J21, and 2) the rather enigmatic question of J14. Neither process involves the 'I'.

- J21 "The symbolic lovers of the free Hellenic world are replaced here by something different"¹
 J14 "What is resumed in the word of Alexandria?"

Furthermore the active participant is the grammatical subject of virtually every main clause with the possible exception of J21. Here we see an overwhelming sense of activeness. The passage starts with two very active and assertive statements:

- J1 "The sea is high again today"
 J2 "you can feel the inventions of Spring"

Main processes in which the 'I' is the explicit actor and subject are all active, assertive verbs:

- J4 "I have escaped to this island"
 J7 "I have come here to heal myself"
 J8 "I light a lamp and walk about"
 J9a "I return link by link along the iron chains of memory"
 J10 "I have had to come so far away in order to understand it all!"

A look at all the material processes shows us that only 8 out of 54 have as grammatical subject something other than actor and of these only J14 and J21 are main clauses. Even J21 lies somewhere in that fuzzy area between a relational and a material process; that action is taking place here is questionable at best. In every material process where 'I' is a participant it is both actor and subject with five possible exceptions: 1) and 2) in J9b, "us" is first the goal of "used" and then the beneficiary of "precipitated"; 3) in J11a, implicit 'I' is "snatched", but only metaphorically as we all are each night, from total darkness by the stars, in this case Arcturus; 4) in J11b, "us" is the goal in "none of us is properly to be judged" but the actor (the judger) is unstated and unknown and the "us" is negated (i.e. "us" is in effect not to be the goal of the action); and 5) in J15, 'I' is beneficiary of "my mind's eye shows me" but certainly "my mind's eye" is a part of the 'I'.

- J9b "the city which used us as its flora--precipitated in us conflicts which were hers"
 J11a "snatched every night from darkness by Arcturus"
 J11b "none of us is properly to be judged"
 J15 "my mind's eye shows me a thousand dust-tormented streets."

Of all the processes involving the 'I' as a participant, 'I' functions as both subject and active participant 20 out of the 25 times it is involved. The only really passive situation is in J9b. 'I' functions as actor and subject in 9 processes or 36% of the time.

In the second novel, Balthazar, a more passive feeling begins to creep in. Of the 43 clause level processes, 9 are passive voice. The very first process, B2, involves the passive, as does the first process where 'I' is a participant, B12, as the "one" in this clause certainly includes the 'I'.

- B2 "prophets' tombs turned to zinc and copper at sunset"
- B12 "One is plunged out of winter"
- B18 "Am I to be reinfected once more by the dream of it and the memory of its inhabitants?"
- B27a "Dispersed now by time and circumstance"
- B27b "the circuit broken forever"
- B38 "Our view of reality is conditioned by our position in space and time"
- B39 "every interpretation is based upon a unique position"
- B40 "the whole picture is changed"
- B46 "everything will be found to be true of everybody"

Of the 44 material processes, however, only 4 have as explicit actor something other than the grammatical subject: 1) in B2, the "sunset" is turning "prophets' tombs" to "zinc and copper"; 2) in B18, "the dream" and "the memory" reinflect 'I'; 3) in B27, "time and circumstance" dispersed "us"; and 4) in B38, both actor, "our position", and goal, "our view", involve the 'I' at least marginally. While the last three processes have 'I' as goal only the last has 'I' as actor.

Of the entire 28 instances of 'I' as participant, 18 'I's' function as both subject and active participant, while 11 'I's' function as both subject and actor. 'I' is the explicit actor of:

- B16 "I must return to it"
- B20 "I am indulging myself"
- B28 "I had set myself the task"
- B30 "I had turned a key"
- B34 "not that anything I wrote about them is untrue"
- B35 "when I wrote"
- B36 "The picture I drew"
- B49 "I am making every attempt to be matter of fact"

These are not the active verbs of Justine; most border on mental processes and at least two, B28 and B49, foreshadow the uncertain verbs that are to come in Clea.

In comparison, especially to Justine, the main character in Mountolive is being acted upon. He seems to be floating with the events rather than controlling them. The book starts with the passive voice, "he had been sent". Here we definitely find Mountolive as the receiver of the action rather than the actor. Even though this is the only instance of the actual use of the passive voice in the beginning passage, it sets the tone for what

follows and is consistent with the passive feeling of subsequent clauses. A close examination of the first five sentences illustrates this passive feeling. The next major verb is "found himself". This is a cognitive process verb that implies some outside agent and has the connotation of discovery by chance. It is much less action-oriented than other verbs of this type. To 'find' something usually means to come upon it by chance, and to 'find oneself' in a particular situation definitely makes the assumption that 'oneself' was not actively responsible. Webster's New World Dictionary defines 'find oneself' as "to become aware of being". It is in this precise sense that Mountolive "found himself attached to the High Commission". The final process in M1a is "to await". Certainly "to await" something is not a very active action; it implies biding one's time until something outside of one's influence occurs.

In M1b, we have the only really active process of the first five sentences, "he was already conducting himself". In M2, the reader learns that Mountolive was finding "it was rather more difficult than usual to be reserved". We do not learn that Mountolive was excited (an active process) but rather the negative of a more passive process, being reserved. The fish-drive had become exciting but the more direct active excitement of Mountolive is at best implied.

In M3, Mountolive forgets, not a very active process. The one active process in M3 does not have Mountolive as the actor but as the beneficiary of the action: "the wash of bilge . . . had toe-capped his white plimsolls". The environment is acting upon the main character. Sentence M4 reiterates that Egypt caused one "to forget oneself".

Finally in sentence M5, we find Mountolive "blessed the chance letter". 'Blessed' connotes being grateful for something that is beyond one's control. The real actor of the action in this sentence is "the chance letter of introduction which brought him to the Hosnani lands". Something as inanimate as a letter, and a chance letter at that, is initiating action. Durrell could have said "he came because of a chance letter", but he did not. He markedly used the letter as subject and actor, with Mountolive in the passive position of direct object.

- M1a "he had been sent to Egypt"
- M1a "found himself attached to the High Commission as a
sort of scribe to await his first diplomatic posting"
- M1b "he was already conducting himself"
- M2 "it was rather more difficult than usual to be reserved"
"so exciting had the fish-drive become"
- M3 "He had in fact quite forgotten"
"the wash of bilge rising through the floor-boards
had toe-capped his white plimsolls with a black stain."
- M4 "In Egypt one seemed to forget oneself"
- M5 "He blessed the chance letter of introduction which had
brought him to the Hosnani lands"

Mountolive also has less action per se than Justine. Barely one half of the main clauses are material processes and much of the action present does not involve Mountolive. Only 11 of the 60 total material processes have Mountolive as actor and except for M1b, the actions all involve rather unassertive or even involuntary bodily movements: Mountolive coughs (M11), sighs and stares down (M16), listens to his own heart-beats (M25), smiles (M30), turns and rests his cheek (M44), studies Narouz's hairy arms (M45), and finally speaks (M47). In contrast, in Justine 71% of the main clauses are material processes with 'I' as actor of such verbs as "have escaped", "have come", "light a lamp", and "walk about". While all the actors in Mountolive function grammatically as subject, 28 of the material clauses have as actor either inanimate objects or natural phenomena, leaving the feeling that Mountolive is a victim of circumstance where one's surroundings control one's actions.

- M11 "His own cough fled across the lake"
- M16 "Mountolive sighed and stared down into the brown water"
- M25 "he listened to his own unhurried heart-beats"
- M30 "Mountolive smiled"
- M44 "Mountolive turned sideways to rest his cheek"
- M45 "he could . . . study the hairy arms and hands"
- M47 "he spoke to them in their native tongue"

Finally in the fourth novel, Clea, the reader feels a strong sense of passiveness on the part of the main character but less of a sense of passiveness about life in general; more action takes place than in either Balthazar or Mountolive. The passage under study contains six clear instances of the passive voice, four of which have 'I' as subject but passive participant.

- C5 "a memory which had been falsified" ('I' memory)
- C15 "I had been enriched"
- C17 "I had been forced to admit"
- C19b "we artists are driven"
- C21 "I had been caught up in"
- C39 "This Grecian world was already being invaded"

In Clea there is not the one-to-one correspondence between subject and active participant that is found particularly in Justine; the subject is often goal or phenomenon.

'I' is actor of 22 material processes but goal, beneficiary, or even just a circumstantial element in 15 processes. In only 18 of the 79 material processes is the 'I' both actor and subject and then of such verbs as "failed" (C13a), "had passed the last ten days" (C32a), "stood between two landscapes (C33), and "awoke" (C48). The first five such clauses are:

- C7 "All the writing which I had borrowed"
- C8 "How long had I been away?"
- C9a "I could hardly compute"

C9b "I had been living there"

C10 "I had been surrendering myself"

The verbs here are not active and assertive but mostly convey failure and uncertainty. As in Balthazar, many border on mental or relational processes.

Durrell has indeed given us four very different impressions of the same events, and the means by which he does this is not just semantic. He has used every linguistic device at his disposal. He has communicated to the reader a feeling of control or lack of control by manipulating such traditional grammatical structures as subject and object and by interrelating in often subtle ways the relationship of these grammatical structures to such logical structures as actor and goal. Durrell conveys meaning by devices over and beyond the choice of words. He is not unique, however, in using language in this way but only in his conscious ability to do so. The same manipulation of language can be found in everyday texts; the meaning we get from these texts can be as much the result of how something is said as what is said.

¹ The quoted examples contain only the clauses being considered and hence most of these examples are only portions of sentences as written in the text.

Chart I Main Clause

	Total	Ma	Me	V	R
<u>J</u>	42	30 [71%]	4	1	7
<u>B</u>	43	18 [42%]	8	3	14
<u>M</u>	68	36 [53%]	12	8	12
<u>C</u>	58	40 [69%]	6	1	11

Chart II Material Process Clause

	Total	S/A
<u>J</u>	54	46 [85%]
<u>B</u>	44	32 [73%]
<u>M</u>	60	51 [85%]
<u>C</u>	79	52 [66%]

Chart III 'I' or M involvement

	Total	S/AP	S/PP	O/AP	O/PP
<u>J</u>	25	20 [80%]	2	0	3 [12%]
<u>B</u>	28	18 [64%]	2	0	7 [24%]
<u>M</u>	42	28 [67%]	1	0	13 [31%]
<u>C</u>	45	27 [60%]	6	2	9 [20%]

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Grammar and reference in a tagmemic
analysis of a literary text

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"How devilish is this task of reading."
Ortega y Gasset 1959 . 14.

1. Introduction.* Because finding an entryway into a text is so often so difficult, literary critics both in Europe and in the United States have, in the past two decades, frequently turned to various theories of linguistics to provide insights and analytic methods with which to begin to understand works of literature. The theories consulted and adapted have, for the most part, depended on a structural, therefore general, approach to language; the more microscopic perspective offered by the tagmemic theory of Kenneth Pike has not played a major role in the influence of linguistics upon literary studies, as critics have been more interested in broader generalizations about many texts than in particular aspects of a single text (Culler 1975, Scholes 1974). Yet for the analyst concerned with an individual work, Pike's "linguistics of particularity" would seem to offer most useful insights and guidelines. In this paper I demonstrate a particular, tagmemic approach to literature through an intensive parsing of one of the fragments of the Pensées of Pascal.¹

Studying any text from within the tagmemic framework requires that the analyst consider the two perspectives of grammar and reference, two of the three major hierarchies of Pike's theory.² The distinction between these two aspects of language is developed in Grammatical Analysis (Pike and Pike 1977a; cf. also Pike and Pike 1977b); the main difference between the hierarchies is that the grammatical deals with general phenomena and the referential with particular matters. In the grammatical analysis the text is broken down into isolated constituents whose context is provided by the general patterns or structures of the language involved. The referential description answers this breakdown by relating the discourse being studied to the particular context--or better, the "prior text"--of the participant's experiences. Both the general and the particular contexts are necessary if the analyst is fully to live in and understand the discourse; one without the other is, in my view, insufficient.

The text to be analyzed here is a very short fragment which epigrammatically expresses Pascal's thoughts on order and organization:

La dernière chose qu'on trouve en faisant un ouvrage
est de savoir celle qu'il faut mettre la première
(Pascal 1952, fragment 8).

[The last thing one finds in creating a work is to
know which thing to put first.]

The question of order, which Pascal finds puzzling in fragment 8, is germane to nearly any tagmemic undertaking, since there arises an inevitable conflict between the order in which the analysis is carried out and the order in which it must be reported. Although in practice the grammatical and referential studies are most often conducted simultaneously, or at least in alternation, the linear form of the narrative report makes it more convenient to present first the grammatical analysis, followed by the referential description. This linearity, however, should not disguise the fundamental interrelation between the two projects, which must remain in balance if the study is to succeed.

2. Grammatical parsing. The tagmemic formulae listed below represent the grammatical analysis of fragment 8, an Equative Clause Root. The formulae are stated in terms of Pikean four-cell tagmemes, summarizing the four aspects of slot, class, role, and cohesion:

slot	class
role	cohesion

Figure 1. The fillers of the four cells of the grammatical tagmeme.

Since this is the analysis of a particular sentence, the class cell in the tagmemes following is often filled by the word appearing in the text rather than by a construction or a class, as is usual in general tagmemic formulae.³ A list of the abbreviations used in these formulae appears immediately following the analysis.

$$\begin{aligned}
 \text{a. EC1Rt} &= + \frac{\text{S}}{\text{It}} \left| \begin{array}{l} \text{NP}^1 \text{ (b.)} \\ \text{suppresses} \\ \text{Mar/It in EFVP} \end{array} \right. + \frac{\text{P}}{\text{E}} \left| \text{EFVP (i.)} \right. \\
 &+ \frac{\text{Co}}{\text{COS}} \left| \text{TInfClRt (j.)} \right. \\
 \text{b. NP}^1 &= + \frac{\text{Mar}}{\text{Spec}} \left| \begin{array}{l} \text{Art} \\ \text{<la>} \\ \text{>gender} \\ \text{>number} \end{array} \right. + \frac{\text{Mar}}{\text{Ord}} \left| \begin{array}{l} \text{Ordinal} \\ \text{<dernière>} \\ \text{>gender} \\ \text{>number} \end{array} \right. + \frac{\text{Nuc}}{\text{It}} \left| \begin{array}{l} \text{N} \\ \text{<chose>} \\ \text{gender>} \\ \text{number>} \\ \text{person>} \\ \text{antecedent} \\ \text{of } \underline{\text{celle}}, \\ \underline{\text{que}} \end{array} \right. \\
 &+ \frac{\text{Mar}}{\text{Res}} \left| \begin{array}{l} \text{ObRelCl}^1 \\ \text{(i.)} \end{array} \right.
 \end{aligned}$$

$$c. \text{ ObRelCl}^1 = + \frac{\text{Nuc}}{\text{Res}} \left| \frac{\text{ObRelClRt}^1 \text{ (g.g.)}}{\text{ManPh (d.)}} \right. + \frac{\text{Mar}}{\text{Man}} \left| \frac{\text{ManPh (d.)}}{\text{Man}} \right.$$

$$d. \text{ ManPh} = + \frac{\text{Mar}}{\text{Rel}} \left| \frac{\text{<en>}}{\text{>RR>}} \right. + \frac{\text{Nuc}}{\text{Man}} \left| \frac{\text{ParClRt (e.)}}{\text{>RR>}} \right. \\ \left. \text{participle>} \right. \left. \text{participle} \right.$$

$$e. \text{ ParClRt} = + \frac{\text{P}}{\text{Sta}} \left| \frac{\text{Participle}}{\text{<faisant>}} \right. + \frac{\text{Ad}}{\text{U}} \left| \frac{\text{NP}^2 \text{ (f.)}}{\text{U}} \right.$$

$$f. \text{ NP}^2 = + \frac{\text{Mar}}{\text{Spec}} \left| \frac{\text{Art}}{\text{<un>}} \right. + \frac{\text{Nuc}}{\text{It}} \left| \frac{\text{N}}{\text{<ouvrage>}} \right. \\ \left. \text{>gender} \right. \left. \text{>gender>} \right. \\ \left. \text{>number} \right. \left. \text{number>} \right.$$

$$g. \text{ ObRelClRt}^1 = + \frac{\text{Ad}}{\text{U}} \left| \frac{\text{RelPro}}{\text{<qu'>}} \right. + \frac{\text{P}}{\text{Sta}} \left| \frac{\text{TFVP (h.)}}{\text{Sta}} \right. \\ \left. \text{antecedent} \right. \left. \text{is } \underline{\text{chose}} \right.$$

$$h. \text{ TFVP} = + \frac{\text{Mar}}{\text{A}} \left| \frac{\text{SPro}}{\text{<on>}} \right. + \frac{\text{Nuc}}{\text{Pred}} \left| \frac{\text{TFV}}{\text{<trouve>}} \right. \\ \left. \text{>person, number>} \right. \left. \text{supresses S/A>} \right. \left. \text{>person, number>} \right.$$

$$i. \text{ EFVP} = + \frac{\text{Nuc}}{\text{Pred}} \left| \frac{\text{EFV}}{\text{<est>}} \right. \\ \left. \text{>person, number} \right.$$

$$j. \text{ TInfClRt} = \frac{\text{P}}{\text{Sta}} \left| \frac{\text{TInfVP (k.)}}{\text{Sta}} \right. + \frac{\text{Ad}}{\text{U}} \left| \frac{\text{NP}^3 \text{ (l.)}}{\text{U}} \right.$$

$$k. \text{ TInfVP} = + \frac{\text{Mar}}{\text{II}} \left| \frac{\text{Prep}}{\text{<de>}} \right. + \frac{\text{Nuc}}{\text{Pred}} \left| \frac{\text{Inf}}{\text{<savoir>}} \right. \\ \left. \text{Infinitive>} \right. \left. \text{>Infinitive} \right.$$

$$l. \text{ NP}^3 = + \frac{\text{Nuc}}{\text{It}} \left| \frac{\text{Pro}}{\text{<celle>}} \right. + \frac{\text{Mar}}{\text{Res}} \left| \frac{\text{ObRelCl}^2 \text{ (m.)}}{\text{Res}} \right. \\ \left. \text{>gender, number} \right. \left. \text{antecedent is } \underline{\text{chose}} \right. \\ \left. \text{antecedent of} \right. \left. \underline{\text{la première, que}} \right.$$

$$m. \text{ ObRelCl}^2 = + \frac{\text{Nuc}}{\text{Res}} \left| \frac{\text{ObRelClRt}^2 \text{ (o.)}}{\text{Res}} \right. + \frac{\text{Mar}}{\text{Ord}} \left| \frac{\text{NP}^4 \text{ (n.)}}{\text{Ord}} \right.$$

n.	NP ⁴	=	+	<table><tr><td>Mar</td><td>Art</td></tr><tr><td>Spec</td><td><la></td></tr><tr><td></td><td>>gender, number</td></tr></table>	Mar	Art	Spec	<la>		>gender, number	+	<table><tr><td>Nuc</td><td>Ordinal</td></tr><tr><td>It</td><td><première></td></tr><tr><td></td><td>>gender, number</td></tr><tr><td></td><td>gender, number></td></tr></table>	Nuc	Ordinal	It	<première>		>gender, number		gender, number>		
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Abbreviations

Ad	Adjunct	Ord	Ordinal
Art	Article	P	Predicate
Cl	Clause	Par	Participial
Co	Complement	Ph	Phrase
COS	Characteristics of Subject	Pred	Predication
E	Equative	Prep	Preposition
F	Finite	Pro	Pronoun
II	Infinitive	Rel	Relative
	Introducer	Res	Restriction
Inf	Infinitive	RR	Relator - Related
It	Item	Rt	Root
Man	Manner	S	Subject
Mar	Margin	Spec	Specifier
Mod	Modal	Sta	Statement
N	Noun	T	Transitive
NP	Noun Phrase	U	Undergoer
Nuc	Nucleus	V	Verb
Ob	Oblique	VP	Verb Phrase
Oblig	Obligation		

With this analysis, too detailed to be fully discussed here, I have cut Pascal's beautiful epigram to ribbons. Structures are broken up, constituents are isolated from each other in separate, insular tagmemes. But the advantage of parsing a sentence in such detail is that it helps the analyst to know the sentence structure intimately, which in turn often allows him to experience various structural patterns. In this short fragment a pattern of mirrored embedding can be discerned, beginning with the two non-finite verb phrases in the clause root: the participle faisant, in formula e, and the Transitive Infinitive verb phrase de savoir, in formula k. Both non-finite verb phrases

are associated with relative clauses: while faisant is included within an Oblique Relative Clause (formulae c - e), de savoir occurs within a Transitive Infinitive Clause Root which includes a relative clause (formulae i - l). This embedding pattern continues for one more layer: the relative clause of faisant modifies a noun in an ordinal expression (formula b), while the relative clause of de savoir itself includes an ordinal expression (formulae k - n). In Figure 2 the domain of each of these constituents is delineated in order to clarify the pattern of mirrored embedding:

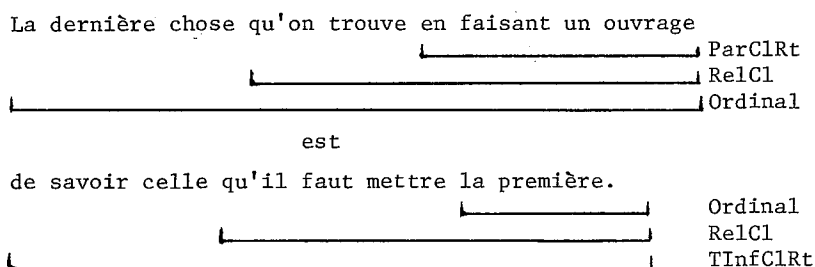


Figure 2. Mirrored embedding pattern in fragment 8.

Associating the Participial Clause Root and the Transitive Infinitive Clause Root as two non-finite verb structures develops the mirrored pattern further: the sequence of the structures of the first half of fragment 8 then appears exactly reversed in the second half, as shown in Figure 3:

Ordinal Expression	Relative Clause	Non-finite V Structure	Non-finite V Structure	Relative Clause	Ordinal Expression
A	B	C	C	B	A

Figure 3. Reversed orders in fragment 8.

While the patterns of Figures 2 and 3 may be pretty, with a balance appealing to one's sense of stability, they are without further significance as long as the analyst confines her study to the bounds of the primarily text-immanent grammatical hierarchy. In order to consider the congruence between the reversal of Figure 3 and Pascal's message of continual reversal, the tagmemic reader of Pascal must begin moving toward the referential hierarchy, for it is on that hierarchy that the more particular factors of perceived purpose and the reader's prior text are treated. With the more personal context of referential concerns beginning to influence the analysis, the reversed order of the grammatical structures in Figure 3 reinforces for me Pascal's assertion in fragment 8 that there is no fixed order in the world: what is once last is at another time first.

A further development of this pattern and its theme is possible if one takes into account the location and function of the copula est in fragment 8: it occurs in between the two sequences ABC and CBA, with the function of linking or equating the first half of the clause root with the second. In effect, est sets up an equivalence, as shown in Figure 4:

Ordinal Expression	Relative Clause	Non-finite V Structure	{ est } = }	Non-finite V Structure	Relative Clause	Ordinal Expression
A	B	C		C	B	A

Figure 4. Equivalence of reversed orders.

Through the copula the reversed orders of these grammatical constituents, units dependent upon the general context of the structure of French, are equated. Again moving toward the referential hierarchy, I derive from this equation an interpretation of the structure of the fragment as an assertion that opposites, such as ABC and CBA, are equal. With the equation of structural opposites, the tagmemic reading leads further into areas of referential concern, in particular the pascalian theme of constant contradiction in the temporal world of misery without God.

The shadings between the referential and the grammatical hierarchies suggest that the two aspects are not separated by a radical gap but are somehow connected. The clearest link between the general and the particular hierarchies is established by general referential structures, one of which is chiasme, a rhetorical device recurring throughout the Pensées, which depends upon a structural order of ABBA. The contradiction ABC = CBA can be manipulated into such a chiasme, if element B, the relative clause, is taken as a pivot point:

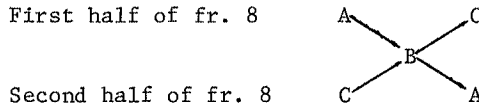


Figure 5. Chiasme of fragment 8.

Associated with the insoluble contradiction from which it was derived, as well as with the Christian cross which it evokes, chiasme is perhaps the most important rhetorical figure for Pascal. With complex connections to prior text, the figure of chiasme moves the analysis completely into the referential hierarchy. But the grammatical analysis cannot be forgotten, since it provides the basis for distinguishing chiasme and other patterns which are referentially interpreted.

3. Referential description. On the referential hierarchy, particular aspects of the language event are treated on several levels, including identity, event, and performative interaction, although in this paper I shall consider only the identity level. It/is here that chiasme is connected to various prior texts and universes of discourse. I consider this figure a general referential structure--a possible oxymoron, since reference depends on particularity--because it is repeated: chiasme occurs at least twice in fragment 8 as well as countless other times throughout the Pensées. The classes of the feminine nominal forms appearing in fragment 8 provide the second chiasme, as shown in Figures 6 and 7:

la dernière	chose	celle	la première
ordinal expression	substantive	substantive	ordinal expression
A	B	B	A

Figure 6. Grammatical classes of feminine nominals.

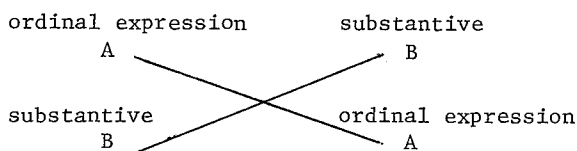


Figure 7. Graphic representation of chiasme of Figure 6.

The first and last of the feminine nominals in fragment 8 are ordinals, while the middle two are both substantives, producing the ABBA order of chiasme.

Because this pattern is repeated, it is general, but insofar as I experience it in relation to a specific context, chiasme is also involved in the particularity of reference. In order to capture the dual connections of a pattern which is general and referential at once, I treat it as an identity on the lowest level of the referential hierarchy.⁴ Like other referential identities, chiasme can be described in a tagmeme array:

	<u>Chiasme</u>
	• ordinal/substantive/substantive/ordinal (Fig. 6)
	A B B A
Figure	• ordinal/non-finite VP/non-finite VP/ordinal (Fig. 4)
	A C C A
Expression of paradox	• Pascal's rhetorical style: chiasme is frequently used. • Pascal's belief in contraries: our world is full of contradictions and paradoxes. • Image of Christian cross: the contraries are resolved in Christ. • All other instances of chiasme in the <u>Pensées</u>.

Figure 8. Referential identity tagmeme describing chiasme in fragment 8.

Although each of the four cells is crucial to the Pikean tagmemicist, at this point in the analysis the cohesion cell, the lower right quadrant, tends to claim greater attention. On the identity level of the referential hierarchy, cohesion deals with universes of discourse within which the identity is related to other identities, situations, or events in the occurrence under study. Of the many possible such universes or prior texts to which chiasme may be related, in Figure 8 I list only a few of the most important. First, chiasme may be considered simply as an element of Pascal's style, one which appears frequently but which may not have greater significance. Secondly, chiasme may be related to Pascal's belief in contraries and in the continual reversals of our worldly existence. In the context of paradox and contradiction, the first thing is inevitably last. Opposites become equal, as in Figure 4, and the ostensibly stable world order is overturned. It is in connection to this kind of prior text that several recent critics, including Louis Marin (1975), have treated the complete Pensées. Yet I find a third possible context: because the cross of the chiasme recalls the Christian cross, I must relate the figure of chiasme to Pascal's view of the crucified and risen Christ as the resolution of worldly contraries.⁵ While it is true that Pascal sees contradiction everywhere in the temporal world, he often expresses paradoxes through chiasme, and the relation of this general referential structure to the universe of discourse of the Christian cross suggests that the contradiction so expressed will ultimately be resolved in Christ.

Because of such differences in prior texts, I arrive at a reading of the Pensées which is quite different from the readings of other critics. While Marin's view of pascalian contradiction as a radically deconstructive force is valid or true within the second universe of discourse shown in Figure 8, that does not lessen the truth of my own conflicting reading of the

resolution of contraries in Jesus Christ, which is itself valid within the third. In tagmemics truth is a referential concern, based on cohesion to a particular context or universe of discourse (Pike and Pike 1977a. 377 - 378). If he recognizes several such universes, therefore, the analyst is free to accept several conflicting truths, a practice which Pascal recommends:

Tous errent d'autant plus dangereusement qu'ils suivent chacun une vérité; leur faute n'est pas de suivre une fausseté, mais de ne pas suivre une autre vérité (fragment 455).

[All err more dangerously insofar as they each follow one truth. Their fault is not in following a falsehood, but in not following another truth.]

The tagmemic reader is not asked to choose one interpretation as true and to discard the other. Instead he is encouraged to accept each as true in relation to a different context or universe of discourse. Recognizing the validity of competing readings permits a more complex, more paradoxical understanding of the work based on plural meanings.

Plural meanings seem necessary if the critic is to deal carefully with all the ramifications of Pascal's contradictory text. Yet there is a danger in allowing conflicting interpretations of a work to stand: an inappropriate emphasis on the apparent equality of the competing views could lead to a weakened style of criticism in which all interpretations, no matter how unfounded, are admitted as valid. The lack of rigour in such an uncritical criticism, however, is countered in the tagmemic approach to a literary text by the requirement that, in the referential description, the reader carefully recognize and consider the context or universe of discourse within which each conflicting reading may be valid. Plural meanings may be accepted, but only on the condition that the context within which each is appropriate be explicitly recognized. The reinforcement and the restriction of plural readings are two of the particular advantages of a tagmemic approach to literary studies.

4. Grammar and reference in balance. One reason Anglo-American critics are sometimes apprehensive about a linguist studying a literary text is that traditional analytic methods of linguistics can lead to the complete segmentation and ultimate breakdown of the coherent work.⁶ While such a breakdown is quite possible through tagmemic grammatical analysis, as the formulae given in § 2 should demonstrate, in tagmemics the referential perspective counters the grammatical deconstruction through its emphasis on cohesion: the general referential structure of chiasme and other patterns discerned in grammatical analysis are not treated as isolated elements but as phenomena with connections throughout the Pensées. Through its cohesion to all other

chiasmues in the text as well as through its cohesion to the resolution of contraries in Christ, the identity of chiasme in fragment 8 is integrated into the larger text. However, neither integration nor isolation alone provides an appropriate view of the *Pensées*. Both grammatical isolation and referential integration are valid, and both should be maintained and accepted simultaneously, if a suitably complex view of the text is to be achieved. Whereas grammatical analysis is not a uniquely tagmemic contribution to philology--since any reasonably sensitive syntactic analysis could provide the same foundation for interpretation--the emphasis on referential integration with prior text is a special advantage of the tagmemic approach to literature, as is the requirement that isolation and integration be held in balance.

5. The difficulty of critical reading. Reading, if one stops to think about it, is hard. Because the task of philology is to make the strange familiar by building bridges from the current to the distant (Becker 1982), that particular style of reading required in criticism is most difficult: becoming involved in the text of the Other makes one aware of the great distance between the Other's conceptual world and one's own. Yet if the goal of reading is to make the distant episteme familiar, the defamiliarization produced by the text must be dealt with. The linguistics of particularity, as Becker suggests, offers one way to address the problem: by further defamiliarizing the text, through the radical segmentation of grammatical analysis, the critical reader becomes aware of the "play of figures and lexical classes at a level below that usually noticed by the literary scholars, and a bit above that usually studied by linguists" (Becker 1982. 8). With this awareness the reader may begin to move inside the formerly distant text, whose structures she now understands intimately, to a more emic or internal view of the world it expresses. In so doing the reader is inevitably changed, as she comes to understand the episteme of the Other, and her own world becomes a little less familiar (Becker 1982. 9). But if the defamiliarization of the reader's world continues unrestrained, he risks losing his own point of view, becoming completely Other, alienating (untying) himself from his own experiences and context. If the philological bridge is to be built between the old and the new, between the familiar and the distant, contact must be maintained with both sides. For this reason the conceptual world of the Other, expressed in the text and understood, at least in part, through grammatical defamiliarization by the critical reader, must be tied in with the reader's own prior text. The relation to his context and experiences cannot be severed completely without destroying the bridge. Because it is the referential description which allows the relation of text with prior text, the referential perspective balances the defamiliarizing grammatical project. Maintaining this precarious

balance is difficult, but in my experience it has been absolutely necessary. Through the double perspective of grammar and reference, the double affirmation of isolation and integration, Pike's linguistics of particularity supports the critical reader in his difficult task of balancing.

Notes

*An earlier version of this paper was presented at a meeting of the Michigan Linguistic Society, October 16, 1981. I am grateful to Alton Becker and to Stephen Tyma for continuing discussions on discourse, tagmemics, and philology.

¹Although in 1964 Pike suggested that his work might prove useful to critics (Pike 1964) and recently broadened and amplified this suggestion (Pike 1981), few have used tagmemics as an approach to specifically literary texts. The principal exception to this statement is Alton Becker, who has adopted the linguistics of particularity as the foundation for his "modern philology" (Becker, forthcoming).

²By leaving out of consideration the third hierarchy, the phonological, I do not wish to suggest that it is irrelevant to the study of a written text. Particularly in poetry, but occasionally in prose as well, phonological analysis can be an important aid in understanding. Nonetheless, the referential and grammatical hierarchies remain of greater importance in the tagmemic approach to a written work, and it is on these two aspects of the pascalian fragment that I concentrate.

³Because general tagmemic formulae in the grammatical hierarchy are intended to be generative, that is, to allow new strings to be generated, the class cell of a general tagmeme is often filled by one or more morpheme classes from which particular morphemes may be chosen to permit generation. Although a particular analysis may also be taken down below word-level to morphemes, I have chosen not to do so here in order to reduce in some small way the complexity of the formulae. Stopping the analysis at word-level does not seem to obscure too much detail, except perhaps with regard to the concord of la dernière chose, celle, and la première.

⁴While for the Pikes the identity level is the lowest of the referential hierarchy, they include in this level only participants and props; the addition of the general referential structure as an identity is my expansion of this level.

⁵I wish to note that the religious views I am describing here are not necessarily my own. Rather than advocating these beliefs, I am only stating their importance for Pascal and, consequently, noting their contextual relevance to a critical reading of the Pensées. Davidson (1979. 123) discusses the

importance of Christ as a "principle of understanding" in Pascal.

⁶Murray Krieger (1976) is quite candid about his reservations about linguistics, for just this reason. However, European critics more often approve of linguistic-based criticism precisely because it allows the deconstruction of the monolithic text. The European perspective depends on a context for reading which is quite different from Krieger's, since it is strongly influenced by the work of Nietzsche, Marx, and Freud.

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THE FUNCTIONS OF LEFT DISLOCATION IN SPONTANEOUS DISCOURSE

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Left dislocation is the construction exemplified in sentences 1-6 in which a noun or noun phrase appears before the beginning of a sentence (i.e. leftmost) and corresponds to an appropriate pro-word in the sentence itself (he in sentences 1 and 2, that in 3, it in 4, them in 5, and there in 6). Left dislocation is not a repair construction which represents a speaker's change of mind and a second attempt at phrasing after having begun a sentence.

1. But this son that lives here with me, he's a strong Democrat. (85-year-old female)
2. And my boy, he froze so bad, he had what they call "buck fever." (53-year-old male)
3. Well, these arbitrary rules that people set up for themselves, that's something else I don't like. (30-year-old male)
4. All that white lightning and everything, you know they got into it somehow. (16-year-old female)
5. Even public records, there's no place to display them. (29-year-old male)
6. Well, at White Pine, you know, you're really more of an individual there. (17-year-old female)

Left dislocation has been analyzed in sentential terms (e.g. by Gundel 1974) and considered a type of topicalization. It has been studied by sociolinguists (as by Tyson 1976, Fitzgerald 1977) and sometimes called "pleonastic pronominalization" and related to the redundant marking of subjects in some varieties of English that is called "double subject." That we are discussing a more diverse phenomenon than double subject here is shown by sentences 3-6. Sentence 3 has that rather than a personal pronoun characteristic of double subjects. It in sentence 4 and them in sentence 5 are objects of prepositions rather than subjects. There is neither a subject nor a personal pronoun in sentence 6.

Left dislocation, I will contend in this paper, is primarily a discourse device that has the general function of establishing themes in ongoing conversational discourse and that has the effect of initiating anaphora chains and thus of creating oral paragraphs. That is, left dislocation cannot be understood without reference to its discourse context.

The corpus on the basis of which I will claim this is a series of forty interviews comprising forty-five hours of tape recorded (approximately 1100 pages of transcript) with a cross-section of the population of a small East Tennessee hill community about forty miles east of Knoxville. All forty speakers, of varying educational backgrounds and ranging in age from 16 to 87, used the left dislocation construction, so that in this paper I will provide a general description of the functions of left dislocation rather than determine any sociolinguistic or syntactic constraints it may have. The 40 speakers in this corpus provided 606 instances of left dislocated NPs. (For a fuller discussion of the forms and distribution of left dislocated NPs, see Montgomery 1979.)

To understand the function of left dislocation in oral discourse, we must determine the relationship of left dislocated NPs to preceding portions of discourse and also to subsequent portions of discourse.

Left dislocation usually involves NPs representing pieces of information which are NEW, that is, not mentioned previously but knowable to or recognizable or assumable by the hearer from outside the immediate verbal context. Of the 606 instances of left dislocation identified in the present corpus, 561, or 92.6 percent of the total, involve such NEW information.² Forty-five instances, 7.4 percent of the total, involve OLD information (which the speaker would probably presume to be in the consciousness of the hearer because it had been mentioned in a closely preceding context; a more detailed discussion and an exemplification of OLD information is given below). These 45 examples of OLD information seem to involve the speaker's giving a special emphasis to the left dislocated piece of information.

Four types of left dislocated or NEW NPs suggest themselves. These types are not mutually exclusive, and the following classification is made only on the basis of the primary character of each of the 606 instances.

Type 1. The majority of left dislocated NPs represent new participants or other pieces of information which a speaker had not previously mentioned at any point. Of the 606, 488, or 80.5 percent of the total, are of this type. In introducing new participants, characters, or other pieces of information to be the nucleus of the ensuing sentence and, usually, sentences, left dislocation is thus a stage-setting device. We see three examples of this in 7 below, a 62-year-old man's discussion of his family.

7. Mother, she's been dead fifty two year, (hmm) and Dad, he's been dead about uh fourteen year. (hmm) So therefore uh, I had a sister, she's been dead, oh, I'd say twenty five year, and I had a half-brother died nine months old, He was a twin, and the other one, he died of meningitis. He choked to death. That's, that's been uh thirty, thirty years, I guess, or maybe a little longer than that, just not knowing the time.

Introducing new information for the first time is thus the primary function of left dislocation. This is true whether the left dislocated NP represents a subject or an object, whether it represents an element in the main or in an embedded clause. Prince 1978 claims that left dislocated subjects tend to represent new information while left dislocated nonsubjects in general represent old information. There is no support for these contentions in the data from this corpus. More than 90 percent of left dislocations serve to introduce new information, regardless of whether they are subjects or not.

Type 2. Another type of left dislocation represents a special kind of new information. The left dislocated NP will name a general class of items, and the following sentence will specify which and/or how many members of the class are involved. In other words, some left dislocations involve a focusing on or narrowing down to a portion of the new information which is introduced. There are 34 instances, 5.6 percent of the total, of this type in the corpus. Two short examples are given in 8 and 9 below.

8. People my age that went north and worked, some of them have come back to the territory. (74-year-old male)
9. These students, I know several of them because I've helped the teacher average their grades.
(17-year-old male)

Type 3. On occasion, a speaker wishes to introduce a piece of information to a hearer abruptly in order to contrast it with a foregone piece of information that was the nucleus of the previous sentence. In such instances, the new information receives contrastive stress and is often preceded by but or however, and frequently it is left dislocated. In our corpus are ten instances, 1.7 percent of the total, which are primarily contrastive and several dozen more whose contrastiveness is evident but secondary. Although usually preceded by one of the conjunctions mentioned above, their primary characteristic is the heightened stress on the left dislocated NP which they have. In example 10 we have contrastive stress on old.

10. . . .of course, as I say, the young ones I don't know. But them old ones, if they liked you, they'd bend over backwards for you. They'd do anything in the world that, for you, if they liked you.
(76-year-old male)

Example 11 has contrastive stress on younger.

11. . . .and I think the older people think there's no use in all this change, you know, Why, we've gone like this for years. Why not continue? (uh-huh)
But I think the uh younger, younger people in town

uh, I think they want change. I, I think they want it to grow and develop, you know.

(39-year-old female)

Type 4. Often a speaker in the course of conversation wishes to reintroduce a piece of information after it has fallen into the background of the conversation. The information may have been mentioned in an earlier turn in the conversation or even earlier in the same turn. But when a speaker wishes to reintroduce it, it is often found in a left dislocation structure. A theme is said to be reintroduced when at least one thematic or nuclear piece of information has intervened since the last mention of the reintroduced one. This intervening material may be from one sentence to many dozens of sentences long. In our corpus 29 of the 606 instances, 4.8 percent of the total, are primarily of this type.

As mentioned earlier, forty-five of the 606 left dislocations represent old pieces of information. When these are left dislocated, they are neither mentioned in the discourse for the first time nor reintroduced after the intervention of a different theme. They are not mentioned to bring something into the forefront of a hearer's consciousness or awareness, therefore; they are mentioned to keep that thematic information in the forefront. In other words, these dislocations represent especially emphasized information. In 12 below, Mrs. Seabrook is left dislocated the second time mentioned (obviously for emphasis) rather than when she is first introduced.

12. . . .they had their own black children went to this school (uh-huh) and their black teacher. And uh I'll tell you a teacher that used to teach over there was Mrs. Seabrook. She died two years ago. Oh Mrs. Seabrook, we all liked her very much. She was, that was, that was a situation that I thought that worked just fine when it came to integration and all like that. (62-year-old female)

Mrs. Seabrook is definitely not new information when it is dislocated, since the teacher was referred to in the two preceding sentences. Old NPs, when dislocated, are often accompanied by interjections such as oh, why, boy, and others.

We have seen that left dislocation is a device which usually introduces new information into discourse. Before we conclude that left dislocation is a thematic device which often organizes following portions of discourse that we can call oral paragraphs, we must see whether the discourse beyond the immediate sentence where the left dislocation occurs is still oriented around that left dislocated NP, which means the use of a pronoun to refer back to that NP or the repetition or paraphrase of that NP. If left dislocation introduces themes, it should be atypical for such NPs

not to be the center of a subsequent discourse organization. To determine this, we will look at a subset of 200 excerpts from the present corpus, five from each of the forty speakers.

In the 200 excerpts, there are 190 instances of left dislocation. Over two-thirds of the left dislocated NPs represent the nuclear or thematic piece of information around which more than one sentence is organized. There are good reasons for the remaining left dislocated NPs to be thematic for only one sentence, as we will see. More than one-half of the total (97 of the 190) are thematic for at least three sentences. Figure 1 summarizes the extent to which the 190 dislocated NPs are thematic and for how many sentences they are thematic.

Figure 1. Extent to Which Left Dislocated NPs Are Thematic

No. of Sentences beyond Introduction for which NP is Thematic.	No. of NPs Thematic to this Extent	% of Total
1	61	32.1
2	32	16.8
3	22	11.6
4	17	8.9
5	14	7.4
6	6	3.1
7	5	2.6
8	8	4.2
9	7	3.7
10	5	2.6
10+	13	6.9
	190	100.0

Example 13 shows a discourse in which a left dislocated NP is thematic for 8 sentences. Here we see the effect of the left dislocated NP, the ladies of our church, in organizing a series of sentences tied together by pronouns (we, us, our) that refer back to that NP.

13. Yes, I usually go, because the ladies of our church, we usually have a booth. (uh-huh) We bake things, and so we have a sale. That's a big day for us, you know, that's a way to make uh some money for our uh projects, missionary projects, and things like that, and that's how we were able to help pay for our uh paint that recently that we uh we painted the inside the church. The ladies did a little painting, but we did have enough cash to buy all the paint.
(54-year-old female)

Example 14 shows how the left dislocated NP that snail darter organized several sentences.

14. . . .that snail darter, that's about the craziest thing ever I've seen. I bet you I've caught a million of them things nearly, out where I was raised. And it just has a little horny head, that's all. They ain't fit for nothing. There's little old Johnson Creek at Maggie Village full of them and stocked with them. (73-year-old male)

Figure 1 shows that left dislocated NPs may be thematic for quite extensive lengths; nearly one-tenth of the 190 instances are thematic for at least ten sentences. The greatest extent to which one is thematic is for thirty-eight sentences.

As a thematic device, left dislocation represents a speaker's strategy of presenting to a hearer what ensuing discourse will be about. It seems unlikely that a speaker would signal the introduction of a theme by such a device which would turn out to be thematic for only one sentence without there being overriding reasons for doing so or an explicit signal that such was the case. Once a theme has been introduced, it seems likely that it would be maintained for several sentences or at least until a speaker signaled to a hearer that it was no longer the theme.

In looking at the sixty-one instances of left dislocated NPs in our corpus which are thematic or nuclear for only one sentence, we find that in the majority (46 of 61) of them a speaker employs one of more cohesive devices (conjunctions and devices to introduce themes especially) to indicate to a hearer that a theme will not be maintained. In the remaining fifteen instances, the speaker provides some other explicit signal.

In eighteen of the sixty-one instances in which a left dislocated NP is not thematic for more than one sentence, we find another theme explicitly introduced in the following sentence. Ten of these involve another left dislocation, two an existential sentence, and six the introduction of another character into the discourse. In twenty-eight instances one or more conjunctions or interjections are used to show the hearer a change in direction. These conjunctions and other structures include and, but, but + then, of course, well, so + then, and others. More often than not, these are preceded by a hesitational uh which reinforces the change of direction they signal to a hearer. Also, they often accompany thematic devices and further strengthen a speaker's change of theme.

What then is the relationship between left dislocated NPs and subsequent discourse? An answer can now be given. Left dislocated NPs are normally thematic for more than one sentence, as usually reflected by a chain of pronouns referring to that NP. After introducing an NP representing new information into discourse through left dislocation, a speaker normally maintains it

as the theme until explicitly indicating otherwise. After a left dislocation, a hearer expects the ensuing discourse to be thematically unified around what has been dislocated. This expectation on the part of the hearer is usually facilitated by a speaker's use of pronominal and nominal anaphora referring to the theme. When a speaker wishes to turn to a new theme, he/she uses one of another device for the hearer's benefit to indicate this is happening. A writer uses an indentation to signal a change in theme and hence the beginning of a new written paragraph; a speaker uses an analogous device to indicate a change in theme and the beginning of a new oral paragraph.

I have tried to show here that what we call left dislocation in English is primarily a discourse phenomenon whose one overriding function, as revealed by the context in which it occurs, is to establish themes in ongoing oral discourse which a speaker normally maintains. It is largely pointless to view left dislocation as only a sentential phenomenon, that is, as only the permutation of normal word order in English for the purpose of topicalization. Left dislocation is only one thematic devices speakers of English use, although a very common one in this corpus. I would go much further and claim that left dislocation is a common strategy of conversational discourse for most speakers of English. Existential sentences are another device of this kind. Ultimately providing a thorough description of such devices will enable us to detail what Longacre has called "the grammar of the paragraph" (Longacre 1979).

Notes

1. The term theme is used here in the sense that Jones 1977 uses it, to refer to the "nuclear constituent" of a unit of discourse. This differs from the use of the term by Halliday and other systemic grammarians, for whom the term refers to a part of the informational structure of individual sentences.
2. The narrow distinction made here between new and old information overlaps with the more common distinction between new and given information (cf. Chafe 1976).

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GREETINGS AND FAREWELLS IN COLLEGE SLANG

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At previous LACUS Forums (1979, 1980), I have used college slang* to address problems of semantic motivation. In this paper I focus on the semantic motivation of greetings and farewells, lexical items which are often called ritualistic or formulaic or idiomatic and which have been studied mainly because of their importance in bounding conversational interchanges. My purpose is to describe what kinds of words and phrases are appropriate to greetings and farewells in general and to examine the slang data in light of such generalizations. To do this I adopt from the field of artificial intelligence the notion FRAME, as proposed by Marvin Minsky (1975).

SEMANTIC MOTIVATION

It has often been noted that for many greetings and farewells the meaning of the expression seems unrelated to the meaning of its constituent parts. The usual conclusion is that such an expression has only FORMAL MOTIVATION: i.e., it uses a recognizable syntactic pattern or recognizable morphemes or words, but the meanings of the forms are ignored. The meaning of the expression seems only arbitrarily connected to the words; in some instances, it is a mystery why the words were ever chosen. We call such an expression FORMULAIC, CONVENTIONAL, or IDIOMATIC. An example is the farewell so long. The meanings of so and long do not add up to, or even contribute to, the meaning which we also convey with the farewell goodbye. Since so and long are English words, and since these two words occur elsewhere in this syntactic order, the expression has some formal motivation; but the use of so long as a farewell seems semantically unrelated to its other uses.

It must be remembered that my concern here is with SYNCHRONIC motivation. Etymological information may clear up the original motivation of an expression, but it does not demonstrate current semantic motivation. And for several ordinary greetings and farewells, even convincing historical explanations are lacking. So long, for example, is sometimes explained as a folk etymology of

*My data come from students in my elementary linguistics classes over the past ten years. Every semester they each submit ten examples of terms they consider "good, current, UNC-CH slang."

Arabic salaam. A less far-fetched proposal is that so long is a reduction from something like May God be with you so long as we are apart. The important point here is that such historical speculations, even if correct, do not demonstrate synchronic semantic motivation. They provide an earlier semantic motivation which serves now only as an explanation for formal motivation. To have current semantic motivation, an expression has to be related semantically in some productive or at least paradigmatic way to other expressions in the language. Current speakers have no knowledge of the possible historical motivations of so long, and no other farewell expressions are clearly related to it; it is, even Ruhl would concede, an idiom.

What is meant by productivity is crucial in classifying an expression as an idiom. Many linguists tend to confine productivity to phrase structure rules and, as a consequence, overlook some widespread but not immediately direct processes of semantic productivity. One such process is ellipsis. Consider the farewell see you. It is clearly related to the longer I'll see you, and both can be elaborated with an additional later, tomorrow, etc. Both are related to slightly altered variants such as (I'll) be seeing you and (I'll) see you when I see you. While not frequent, (I'll) see you when(ever) are also possible. I'll can be omitted because it is predictable -- in transformational terms, RECOVERABLE -- in this context. Similarly, nice to talk to you and good to see you omit it's been or it was. Much obliged and glad to do it omit I'm or I was.

Scholars who would overlook productivity of this sort and still call see you an idiom do so because of two serious theoretical problems. One is the problem I will call UNDERDETERMINATION. What is it that makes an expression a greeting or a farewell? Neither comes with morphemic markers branding it as such. Further, while not everything can serve as a greeting or a farewell, there is nevertheless a bewildering variety of possibilities for each. Consider farewells, for example. I'll see you is directed toward the future; it was nice to see you toward the past. Both are statements; take care and good luck are requests, one directed to the auditor, the other to God or fate. Both of these requests are welfare wishes for the auditor, but remember me to your husband is a request for remembrance to the auditor's family members. The response I will is a promise. It is also unclear whether all of these should count as farewells, or whether we should distinguish between farewells and final remarks. Alternatively, we can distinguish between those expressions which seem to be inherently farewells -- that is, are not used for any other purpose -- and those expressions which serve other purposes but can be adapted or understood in context as farewells. But the reason why these possibilities must be discussed is that there is no obvious, purely linguistic indicator which makes an expression a farewell. Such a status appears arbitrary, and that is why some scholars call them idioms.

A second serious theoretical problem is what I will call RITUALIZATION. Jeff Chamberlain of The University of Louisville

has given me an excellent example. He told me of a conversation he observed between two people, one of whom was moving far away; they would not see each other in the foreseeable future, and their conversation ended discussing this regretted prospect. When they parted, both said in farewell see you. Here this expression is used in a way inappropriate to the circumstances. Mike Dressman of The University of Houston has told of a woman who habitually uses Fine as a response even to greetings like Hello. In such instances, speakers seem to forget what the expression is saying, thus making its semantics irrelevant; again, it seems to some scholars that expressions so used should be considered idioms.

Such ritualized expressions illustrate another difficulty in semantic analysis -- that the meaning conveyed by the use of a linguistic form can arise not only from the form itself but also from attendant extralinguistic factors. As Dwight Bolinger has noted, meaning conveyed extralinguistically is often wrongly attributed to lexical items, resulting in what he calls SEMANTIC OVERLOADING. Charles Ruhl has been making the same point in a series of papers. I find greetings and farewells to be an excellent example of expressions that convey meaning extralinguistically. Speakers have a store of knowledge about opening and closing conversations and the appropriate actions for each. If this store exists as a common ground for both speakers and hearers, then a speaker need utter only a brief, minimally informative linguistic expression, which will convey more than it actually says because it activates the store of knowledge. From this standpoint, underdetermination becomes the normal, expected eventuality. To characterize this store of knowledge for greetings and farewells I adopt the descriptive device used in the field of artificial intelligence known as a FRAME.

FRAMES

Put simply, the notion of Frame is intended to contribute to the goal of providing computers with organized methods of interpretation in situations of incomplete knowledge -- to give them, in other words, "common sense."

To illustrate how such methods are used by human beings, let's consider a conversation between you and me in which I say, I wanted to go to the party, but I was out of gas. No English-speaking American is likely to have any difficulty interpreting this sentence, even though I did not mention something significant, namely, an automobile. The assumption by scholars in artificial intelligence is that humans supply contexts for sentences like this by drawing on an organized fund of knowledge of typical objects, events, activities and institutions and that they have strategies for knowing when particular stores are relevant in a particular situation. If confronted with this sentence, the computer must be able to deduce that a car is often used in going to a party and that it needs gas. A Frame, then, is a means of providing a context.

The contexts a human would apply in this situation are quite varied. Note that by saying THE party, I assume that the hearer has a means for determining which party I am talking about. Moreover this Frame and the automobile Frame concern only rather "narrow" contexts. Frames which provide "wide" contexts would enable the hearer to wonder about how I manage my affairs (Why did she let the car run out of gas?), about the extent of my friendships (Couldn't she ask someone to take her to the party?), and even about my motivations (Maybe she didn't want to go to the party, and this is just an excuse, possibly fabricated). Hearers also have Frames for determining when it might be possible to voice some of these thoughts. In short, the notion of Frame can be used to describe any store of knowledge that humans have about typical situations. Ideally, Frames should also enable the computer to make adjustments when the situation is not typical.

Although there are many "wide" Frame features of greetings and farewells that concern the identities of the interlocutors and their social and psychological needs (as described by Erving Goffman 1971, ch. 3), I am here concerned only with those "narrow" Frame features that help to explain the semantic appropriateness of constituent words. This may at first glance look like a misguided enterprise. Charles Ferguson (1976) describes greetings as "ritualized interpersonal exchanges" (138), which are "non-referential in meaning and important for their presence or absence on the appropriate occasion rather than for the exact meanings carried by their constituent parts..." (147). This description is apt, but constituent parts are not irrelevant. Except for single words such as Hello, which has no constituency and no reference or meaning other than its appropriateness as a greeting, the words that constitute a greeting or farewell can be seen to be semantically appropriate, in spite of the general classification of expressions such as How do you do? and So long as idiomatic. There are, for example, differences in appropriate use for Good morning, Good afternoon, and Good night that relate to the meanings of morning, afternoon, and night; and while Good morning may be treated as pure formula, evoking Hi or Hello as a response, it is also possible to get the response What's good about it? (Ferguson, p. 141). Similarly, How are you? can be treated as a formula, evoking Hi or Hello in response, but it can also get Fine. And Fine can itself be formulaic, uttered for form's sake when the speaker feels terrible.

The point of a Frame is that it will provide a computer with the human ability of knowing how to recognize when something is intended to be a greeting or farewell; the Frame must supply as many details as possible to account for the range of possibilities. Quite obviously, my proposals are tentative and certainly would not give a computer the full human ability; my purpose is to demonstrate the possibilities of a Frame as an explanatory device, so that it can stand as either a point of departure or rejection for further research.

GREETINGS

To begin my analysis, I first make some Frame statements about greetings and then elaborate on and exemplify them. I then relate these observations to slang.

Some typical characteristics of greetings:

GREETING-FRAME

1. Purpose: To initiate a verbal exchange which acknowledges the presence of someone else and invites a response in kind.
2. Implied personal relationship: Ranging from minimal (with perhaps no further speech) to intimate.
3. Content of utterance: Very little information, perhaps totally perfunctory and ritualized, referring only to present situation and general, ambient conditions.

Linguistic reflexes:

1. Sentence form: Since a response is invited, questions are most likely, imperatives next, and declaratives least.
2. Utterance length: Extremely short, perhaps only a phrase or word; ellipsis and clipping of utterance highly likely.
3. Pronouns: Ambient it highly likely; you possible, I very unlikely. Any noun phrase with force of ambient it is possible. Interrogatives how and what very likely, the others very unlikely.
4. Verbs: General, non-informative, referring to general conditions: be, do, go. The shorter the better: be, but *exists. Tense: present; simple present likeliest, then progressive, then perfect. Possibly, a reference to the future.

Summary statement:

The shorter the expression,
the shorter the words used,
the emptier they are of content,
and the less prominent they are phonologically,
THEN the more likely it is that an initial utterance is
intended as and interpreted as a greeting.

Of the sentence forms, questions are likeliest because they imply a verbal response; imperatives imply a response, but not necessarily a verbal one; and declaratives do not imply a response. Thus, the frequency of question forms in greetings is to be expected. Considering the wide range of implied personal relationships, certain verbal possibilities must exist for the more perfunctory and ritualized (that is, impersonal) situations,

and the question How do you do? has acquired this status. But even though it is ritualized, it is predictable in form and content by the Frame: the first do serves as auxiliary support in a question, and the rest of the content inquires generally and concisely about the addressee's current status. How goes it? is the ritualized counterpart for inquiring about ambient conditions in general. Of course, declaratives can be used under appropriate conditions: (a) If someone is hedging a greeting, as in (It) looks like rain, the declarative allows the option that the addressee will ignore it (of course, it can be turned into a question with the tag doesn't it?); or (b) If the addressee is known, then the greeting may merge with the first part of the conversation proper, as in (It's) good to see you (again). Since a response like Hello to this is odd, but not impossible, this expression is also somewhat ritualized, as are also the reduced imperatives Good morning, etc.

Personal pronoun choice reflects the purpose of the greeting and the implied relationship. Since the purpose is to acknowledge the addressee, you is possible, but too much emphasis on it makes the implied relationship more intimate; it may be safer to avoid you and use an ambient it expression. The use of I imposes a close personal relationship, as in As I live and breathe, if it isn't Freddy. The interrogative pronouns how and what are relatively empty of meaning in greetings, but the other interrogatives tend to make an utterance more a request than a greeting. Since the greeting is made in a context which has no past and may have no future, present tense is the only reliable possibility; and among verbs, only the most general in meaning and most inconspicuous are servicable in the more formal situations. However, in situations with more personal feeling, all of these conditions may be relaxed, to the point perhaps of dispensing with a greeting entirely.

I turn now to the expressions that function as greetings in my corpus of slang.

What's happening?	What it is?
What's jumping?	Where it's at?
What's the deal?	'S up?
What's going down?	Chello
What's up?	Check in
What does it look like?	Come in, Berlin
	Yo (, Baby, yo.)

To understand a slang expression, a computer would need additional information. Since slang assumes an in-group, with more personal interaction, a slang context has the effect of making speech less perfunctory, or of requiring the perfunctory to be expressed in more informal terms. With informality, the linguistic constraints just noted can be relaxed; that is, the requirements of the Frame can be departed from creatively. Since the purpose of an in-group is in part to exclude outsiders, any creative elaborations may produce an effect which will be opaque to the general public, although this is not a requirement. Con-

sider now the greetings listed above. The first column has all questions, and all inquire about general, ambient conditions; but each one has some creative innovation. Happen is the choice of a longer, more conspicuous word; jump is a metaphor for the same meaning. Both expressions also use progressive aspect. The deal is an elaborate substitute for ambient it. The next two expressions exploit English uses of go, come, up, and down as they are applied to Time. Go suggests 'from the present to the past' and come 'from the future to the present'. Up suggests 'current' and down 'no longer current', but in a slang context both of these words may acquire suggestiveness from other, even unrelated, expressions. Go and go down apply to events just past, go on to events now in motion, and come, up, and come up to events soon-to-be. But in the context of greetings these distinctions have no practical force, and they can be considered interchangeable. What does it look like? is even more elaborate. But all of these expressions are simply variants of the standard greeting pattern of asking about ambient conditions.

Some of the expressions in column two reflect minor tinkering. What it is? and Where it's at?, which are also questions about ambient conditions, exhibit a failure to apply subject-verb inversion in questions, a syntactic trick quite common in slang. Where...at reflects a current semantic innovation to depict nonspatial, even mental or "soul", states in spatial terms, as in This is where I'm coming from 'how I view things'. 'S up is a reduction of What's up?

The other expressions are more mysterious. Chello may be simply a phonetic distortion of Hello, or it may be a blend of Ciao and Hello, Ciao being a slang adoption of a foreign expression. Chello has the companion Chbye as a farewell. Phonetic distortions, blends, and "non-learned" adoptions of foreign terms are all common processes in slang. Check in is paired with several check farewells (check you, check you later). Come in, Berlin is rhyming slang addressed to someone who is day-dreaming or otherwise "out of it." These imperatives imply a more personal relationship. Both come and in are obviously appropriate for a situation which initiates personal contact. Check in and come in appear to be generalized to apply in more situations than in normal usage.

The final expression, Yo, has probably been around for a long time. It has no constituency to account for. I include it because my students report it as a general all-purpose word. It is not only a greeting, but also a farewell. It can also signal agreement or substitute for Come in in response to a knock on the door. Yo, Baby, Yo is an expansion used as a greeting on campus among the members of one fraternity.

FAREWELLS

I now propose a Frame for farewells.

FAREWELL-FRAME

Purpose: To enable two people to acknowledge verbally their departure from each other

Implied personal relationship: Ranging from minimal to intimate, civil to friendly. A preceding period of togetherness, usually with speech.

Content of utterance:

1. Value expressions: favorable to very favorable
2. Time references:
 - a. past (past tense or perfect aspect, often elliptic)
 - b. future (often elliptic)
3. General speech acts:
 - a. statements (to auditor)
 - b. requests (to auditor or providence)
4. Specific speech acts:
 - a. gratitude-thanks-appreciation-praise
 - b. wishes for luck and welfare to auditor
 - c. remembrances to auditor's family members (implying remembrance of them)
5. Topics: immediately previous or expected next togetherness of speaker and auditor
6. Intended impression on auditor: anything which makes the auditor feel appreciated within the limits imposed by the nature of the relationship.

Linguistic reflexes:

1. Sentence form: imperatives and declaratives; no questions
2. Utterance length: extremely short, ellipsis highly likely
3. Pronouns: it 'period of togetherness'; you possible but usually implied; I less likely.
4. Adjectives: positive values such as nice, good, great
5. Other key words: expressions of future such as later and tomorrow; verbs such as see, meet, talk, indicating activity of relationship; God.

I have not followed exactly the same format as for greetings. One difference I discovered is that it is much easier to specify linguistic reflexes for greetings than for farewells. Thus, for farewells, I develop the Content of Utterance section much more. Of course, this difference may simply be the result of my uneven insight. Some specific points should be stressed for farewells. First, although questions are possible for greetings, they seem impossible to me for farewells. This seems evident given the different nature of the two situations; a question urges a response, something required in a greeting but not in a farewell. Second, positive valued adjectives such as nice, great, and good are conspicuous in farewells; these are used to comment on the period of togetherness of the two speakers, a period which for politeness's sake must be spoken of in laudatory terms. Express-

sions of praise, thanks, and appreciation fill the same function. Comparable functions are absent in greetings. Third, the topics spoken of are the immediately preceding period of togetherness and the future; the latter includes as possibilities the next meeting of the two participants and their future welfare. In short, given the speech situation in which farewells are used, the expressions used contain information which relates very appropriately to a small set of natural attendant circumstances and expectations, for example It's been great, I hope we meet you again sometime, have a good day, It was nice running into you, give Sara a kiss for me. It is not hard to create farewells almost at will: looks like it'll be a nice day, I'll probably be at your talk tomorrow, and I hope your wife enjoys her new job. Some are humorous: Don't take any wooden nickels. Farewells can also show appreciation in a seemingly hostile way, as when Colonel Henry Blake, upon leaving the M.A.S.H. unit, says to Radar, Behave yourself, or I'm going to come back and kick your butt.

The class of possible farewells is thus quite extensive, perhaps even open-ended. This means that farewells are not linguistically identified as such nor do they need to be. In fact, the notion of 'farewell' is only SUGGESTED by the language. The hearer can infer from what is said and from the situation that the speaker intends a farewell. If we take "farewellness" as a situationally and inferentially derived notion, then the problem of ritualization can be addressed in a more insightful way. It is not the language, but the farewell situation itself, which has been ritualized. To deal with such situations, we need linguistic possibilities for those times when we give little or no thought to what we say, when we act only for form's sake, or when we want to be noncommittal. Thus, we would expect for any ritualized situation that certain expressions would come to fill the need. For farewells, so long, goodbye, good evening, good night, and see you all have this status, and thus all can be used in what appear to be inappropriate situations. But while ritualization can produce truly idiomatic terms such as so long, it can also attach to expressions which continue to be related to other parts of the language: good evening and see you. In short, ritualization should not be a test for idiomatic status.

With these observations and the farewell Frame before you, let me now address the slang terms in my data that students listed as farewells.

Catch you later
 Check you GREETING: Check in
 Check you later
 Check you on the flip side
 Later
 Later, tater
 It's been real
 Chbye GREETING: Hello
 G.B.
 Caio
 Luego

I have not received as many farewells as greetings over the past ten years, but I have no clear idea why. The terms listed, however, provide several insights into what I have called the innovative nature of slang. When we supply frames to the computer, we must tell it that in slang speakers depart from normal usage according to particular strategies, sometimes involving the suspension or creative extension of procedures and expectations that obtain in general usage. The verb see appears often in farewells, referring either to the immediately previous togetherness or to an expected meeting in the future; meet is a common variant. In slang, the same notion is conveyed by other verbs, such as catch or check in the first four examples. The use of check here probably parallels its use in the greeting check in 'get in touch'. Later is quite common in farewells, referring again to that expected next meeting. On the flip side occurs in place of later in the fourth example. Later also appears as a farewell by itself in slang; and a favorite device in slang, rhyming, extends it to later, tater. Instead of it's been nice or it's been great, where it refers to the just completed period of togetherness, slang uses the structure with the innovative positive adjective real. Phonological tampering in slang is illustrated by chbye, which forms a pair with the greeting chello. G.B. is an acronym for goodbye; acronyms are also a favorite device of slang. Caio and luego are farewells borrowed from Italian and Spanish; college slang sometimes borrows from current foreign languages, for example, legumes from French.

In summary, the notion FRAME can be used to describe linguistic and extra linguistic features which are appropriate for the acts of opening and closing verbal exchanges. Analyzing the data this way extends the notion of productivity and allows us to see that greetings and farewells do have synchronic semantic motivation. The greetings and farewells of slang are also, for the most part, predicted by the Frames; and when they do depart creatively from the features of the Frame, they do so in ways consistent with the productive processes of slang.

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VII

BILINGUALISM

ON CERTAIN BASIC FLAWS IN THE CRITICISM
OF MODERN BILINGUAL EDUCATION

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In his monograph on "The Social Science Perspective" for the 1977 Bilingual Education: Current Perspectives series, Joshua Fishman remarked that "few [if any] integrative efforts along rejective lines have appeared in print (attacking the validity or desirability of bilingual education)" (p. 6). During the past five years, however, a number of purportedly impartial criticisms have appeared on the social and economic value of bilingual education (by John R. Edwards and Noel Epstein), and as well two scientifically stringent, not to say rigorous, evaluations of the methodology and purpose of bilingual education have emerged in the AIR report (see Danoff, 1977) and in the Baker-de Kanter (1981) analysis for the current administration's Department of Education. All these critical responses have serious flaws, and it will be the purpose of this paper to characterize these and discuss their significance.

Before we inspect the flaws in these criticisms, it is important to review what those in the field of bilingual education have believed about their discipline for some time in the past, and as well what they are coming to believe in this decade. Among the deepest motives for all educators in this country has been the desire to reduce illiteracy (in English or any language); current estimates are that 10 percent of the United States population is functionally illiterate. Further, educators have long followed a mission to prepare majority and minority population groups to participate in modern economic life. Since the publication in 1953 of the UNESCO report on The Use of Vernacular Languages in Education, experts on bilingualism have adhered to the axiomatic point of view of that report, which held that members of any minority population should be educated to the point of literacy in their mother tongue. Most of the mainstream research on this topic (including both pro and con attitudes) has emphasized that a language policy which fails to educate for literacy in the mother tongue will produce high drop-out rates and subsequent problems of unemployability and crime among minority populations (see, for example, Fishman (1972), Lewis (1980) and Drake (1975)).

Specialists in language acquisition research have shown that children in their sixth, seventh, and eighth years undergo a critical phase in their cognitive development; during this time, in Piagetian terms, children are in a pre-operational phase in which they first connect the meaning of abstract concepts to specific words in their mother tongue. According to bilingual education experts this time in early cognitive maturation must be held

inviolate: for purposes of essential emotional and intellectual health, a child's mother tongue should and must be protected through the child's education in the third grade. When the fourth grade begins the child is literate, and with a crucial respect for one's mother tongue, one's culture, and oneself, the child may then start a program of transitional bilingual education to the socially or economically dominant language in the country in question; by the end of the sixth grade the child will be bilingual and biliterate, and will probably score higher on standardized verbal and mathematical tests than will monolingual students of the same age (for reports of successful programs, see Troike 1978, Rosier and Holm 1980, and Cummins 1981).

The theater in which healthy, literate, productive, and employable bilingual people are acting out their roles is part of a larger drama in which themes of nationalism and ethnic development are at play as well. Modern social science research (see Deutsch, Eisenstadt, Kelman, Allardt, and Van der Plank) has shown that there is a natural developmental sequence as ethnic groups mature and modernize in the twentieth-century economy. As linguistic minorities come to participate in urbanized, industrial life, they seek to legitimize their vernacular and their pre-industrial culture through an emphasis upon schooling in their mother tongue and in their ethnic literatures. This phenomenon has been observed and accounted for across the face of Europe; and whether Americans like it or not,¹ it is developmentally natural for ethnic minorities to preserve sanctions for their mother tongue. This particular social science perspective, then, emphasizes that public support of bilingual education may be unavoidable; furthermore, in that ethnic maturation² allows individuals a recourse away from the bland WASP culture, it is certainly the case that the new ethnicity contributes to a sense of personal health and greater productivity. Thus, one may argue that to link oneself with an ethnic past, and to sustain a concurrent interest in bilingualism, is actually good for the economy. In all political and social practicality, however, as bilingual education is implemented in the United States today there are according to Jose Cardenas an "insignificant number of maintenance programs," (Epstein 1977, p. 73); instead the "vast majority" of bilingual education schemes emphasize ESL training, and there are to be found a "limited number of transitional bilingual education programs" (Epstein, p. 73). The federal funding allotted to support bilingual education in fiscal year 1981-82 was \$144 million, a figure which, as I have pointed out elsewhere (Donahue 1982), is less than the amount the government spends on tank ammunition for the U.S. Army.

The first critical response to this group of sophisticated analyses, research results, and facts involved, so to speak, cutting out any available member of the herd and dashing in for a slavering attack. Thus Edwards (1976), having no data, attempted to state just any "sensible view" (Fishman 1977, p. 2): a combination of "concerned individuals in government" and "local University people" (Edwards 1976, p. 71) was responsible for the

Elementary and Secondary Education Act of 1968. At this early stage in his writings Edwards also introduced the theme that educating minority students separately may be improperly segregating them (1976, p. 71); he then suggested that by using bilingual education to encourage cultural pluralism, the government fosters anomie: in a place like Cleveland, for example, the government could close off a person's desires to be less Polish and more General American (1976, p. 74). This is Edwards' interpretation of anomie; instead of normlessness occurring, in Durkheim's sense, when traditional society breaks apart, we now see that people become disoriented when their traditional ethnic values are re-introduced.

Noel Epstein continued the attack in 1977. He, too, spoke of "social separation" (p. 4) and "segregation" (p. 23) in bilingual education classes; indeed, Epstein implied that educators gloried in the separation of their students because this kind of discrimination is "a powerful reinforcer of ethnicity" (p. 34). As Epstein saw the issues, bilingual educators were not professionals, but instead "advocates" (p. 14) and "supporters" (p. 15) who in their push for language maintenance (he felt most programs were maintenance- and not transition-oriented) were establishing a policy of "affirmative ethnicity" (p. 3) at immense public expense. Epstein, who was unable to evaluate the program and who knew only an early draft of the AIR report, insisted that there was "no reliable evidence" (p. 6) to support bilingual education, and believed that the whole undertaking seemed unAmerican: it promoted a divisive pluralism in which individuality was submerged (p. 46). Lastly, Epstein charged (without proving it) that many bilingual educators are in the field merely to exploit the available public funds for teaching jobs (p. 29) (one might think that to be consistent we should admire their initiative and enterprise.)

In later articles, Edwards (1977, 1980a, 1980b, and 1981) has darted in to maul the bilingual education carcass by re-emphasizing his earlier criticisms and Epstein's as well. In summary, the developing thrust of his remarks is as follows: the advocacy of cultural pluralism is part of the hidden agenda of bilingual education experts (1980a); no one has asked the general public whether they want any of this, and activist politicians make sure that they're getting it whether they like it or not (1977, 1980b, 1981); ethnic leadership-elites willfully confuse facts and values about modern minority groups in order to impose their social policies upon the general public (1980b, 1981); when bilingual education is used to foster ethnic values, educators have suppressed "negative findings" about it (see 1981, p. 28), for transitional bilingual education tends to move people into the de-ethnicized mainstream anyway (1981, see p. 30); further, the promotion of ethnicity and cultural pluralism may be non-democratic (see 1981, especially p. 34) and thus not in the best national interest, for it can invite tyranny (1980b, 1981)³; and lastly, "a special ethnic group" is behind it all (the context implies that he is speaking about the Jews: 1981, p. 40). I dearly hope that even after looking at that section dozens of

times, I am misreading that last point.

A fair-minded person's first response to all this is, to use Epstein's phrase against him, "many different and complex issues have been blurred together" (1977, p. 49). We pass over the psycholinguistics expertise of a scholar like James Cummins at our peril (recently he discussed research results which defend language maintenance, showing that "the cognitive advantages associated with bilingualism have involved students whose L1 proficiency has continued to develop while L2 is being acquired" (see Cummins 1981, p. 38 and the bibliography of that article)). Yet even should we disregard the work of language acquisition experts, we must keep in mind that the arguments for bilingual education range from psycholinguistics across the fields of sociolinguistics and social and political science. Thus the basic flaw in the approaches by such critics of bilingual education as Edwards and Epstein is that with their peculiar emphasis on the dimension of ethnicity they narrow their argumentative focus irresponsibly and, in the rhetorician's sense, they divide the question inaccurately. At that, they draw upon a modern school of scholarship on ethnicity which has questionable solidity and profundity. One of Edwards' favorites among the historians of the new ethnicity, Thomas Sowell, seems wholly ignorant of the facts of first and second language acquisition; the news that psycholinguistics exists at all might come as a shock. Sowell, a theoretician of the capitalist-meliorism school (taking the position of a de-Calvinized Max Weber) characterizes culture in terms of "human capital" (1981, p. 282); in this context, ethnic groups in America are "hampered" by a "lack of English" (p. 283), and instruction in one's mother tongue has nothing to do with the "effort, thrift, dependability, [and] foresight" (p. 283) necessary for economic success in America.

Sowell is only one member of a recent school of writers on ethnicity. Others of this persuasion move slightly beyond the insight that wallowing in ethnicity stands in the way of material success. Orlando Patterson (1977), for example, remarked that an interest in ethnicity was a "return in social thought to both romanticism and Platonic idealism" (p. 29) and it is foisted on people who yearn for "the lost paradise" as part of a "sorcerer's tradition" (p. 25): there is a consequent loss of individuality and a new desire to return to the "preindustrial cradle" (p. 148) of an agrarian ethnicity that has all "the richness of a north-west indian potlatch" (p. 160). Stephen Steinberg (1981) sees modern ethnicity as dominated by empty symbols, with an emphasis on "popular culture" and an "objects culture" (p. 61); ethnic mobilization originally was a reaction to "conquest, slavery, and exploitation of foreign labor" (p. 5), and in modern times it is out of place with the current capitalistic economy which naturally assures mobility, opportunity, and access--and a consequent de-ethnification (p. 260).

The difficulty with such modern views of ethnicity, and with their impact on such critics of bilingual education as J. R. Edwards, lies in the fact that they are caught in a post-World

War II time warp. Orlando Patterson, for example, feels that the resurgence of interest in ethnicity derives from a "crisis of values" after the Vietnam War, when various groups allowed their thinking to be snagged by "hedonistic escapism, spiritual revivalism, semi-pathetic reengagement, and ethnic revivalism" (p. 161). Such views can only arise in neglect, disregard, or ignorance of the classical formulation of the connection between ethnicity and anomie (see Beer, 1980). If we look at what ethnicity meant, not just since the Vietnam War, but instead since the 1840s, we see that ethnic groups existed a priori in the emergence of the capitalist economy, and that in the sense discussed by Geertz (1963) ethnicity is a primary cognitive mode through which the individual learns about the world and attaches meaning to the society and economy around him. One has to ask why we aren't shown the argument that ethnicity is the central modality through which individuals, groups, and nations participate in the real world. Indeed, where are the classical thinkers in this body of doctrine? Won't you trot out your heavyweights, please? If one persists in an ethnicity-based criticism of bilingual education, one should show that he is informed by Pareto, Durkheim, Weber, Deutsch, Eisenstadt, Fishman, and Hechter as well, at the outset.

The first flaw, then, in modern bilingual criticism is the essential narrowness of its base of complaint. The second flaw develops in its tone and manner--with the result that the average informed citizen cannot rid himself of the suspicion that the criticism is at its base politicized and on the surface polemically motivated. In the case of the AIR report, an analysis of 38 American programs for bilingual education in Spanish and English which was released in 1977 by the U.S. Office of Education, serious questions emerge concerning the motivation behind the analysis of these Title VII programs. Among the charges made by Professor Cardenas against the AIR report (in Epstein 1977, pp. 74-75) is the fact that the American Institute of Research characterized most of the programs to be of the "maintenance" type; yet after using this emotional buzzword, the report did not define what it meant by "maintenance." The report in general showed that students in Title VII programs did not show achievement gains which could actually "be attributed to bilingual instruction" (as Epstein phrases it, p. 11); yet as Cardenas points out, the administration of the programs in question had so many problems that in the final judgment nothing pro or con could be concluded about the effectiveness of bilingual education at all (Epstein 1977, pp. 74-75).⁴ Thus it was indeed time for a rigorous statistical analysis of bilingual education programs, and in 1981 the Reagan administration released one.

The report entitled Effectiveness of Bilingual Education: A Review of the Literature (1981) was written by Keith A. Baker and Adriana de Kanter, who are described as "staff members in the Office of Planning, Budget and Evaluation" (1981, title page); we are given to understand at the beginning that "the report does not represent the official position of the U.S. Department of Educa-

tion," although of course that particular entity is not destined to be around long enough to sanction anything. Baker and de Kanter undertake their analysis with the following rationale: "the basic objective of scientific research is to rule out alternative explanations" (Chapter I, p. 9). Understood in an historical perspective, this axiomatic approach is an application of what came to be known in the fourteenth century as Occam's Razor: entia non sunt multiplicanda praeter necessitatem, or entities should not be multiplied except from necessity. Such an analytical technique works best not when one compares competing arguments, but instead when one defends a pre-established principle; this may all be well and good, but everything depends on what the initial point of view is. In linguistics, for example, this technique could be used to eliminate all the scholarship of Sir William Jones and everybody who followed him. In any case, this research principle has an outstanding built-in flaw: investigators can arrive at no concluding proposition other than the one they may have started with.

Baker and de Kanter's case-study analyses were indeed carried on with an impressive rigor. They primarily wished to ask:

1. Does transitional bilingual education lend to better performance in English?
 2. Does transitional bilingual education lead to better performance in non-language subject areas? (Summary, p. 11).
- For answers to these questions they reviewed "over 300 documents" (Abstract, p. 1) in an attempt to find out first, if the study presented "data relevant to the issues of interest," second (note the intriguing wording) whether the "design of the study permit[ted] any plausible alternative explanation for the results," and lastly, "how widely the results of the acceptable studies [can] be generalized" (Summary, p. 3). Studies were rejected if they did not "address the issues" (I, p. 11), or if they showed "nonrandom assignment with no effort to control for possible initial differences between control and program groups" (I, p. 11), or if the performance of minority group students was compared in any way to that of majority students in national norms (I, p. 13), or if there was a "comparison of posttest scores only, with nonrandom assignment" (I, p. 14), or if the study measured "school-year gains only, without a control group" (I, p. 14) or, lastly, if the study measured achievement through the use of grade-equivalent scores (I, p. 15).

We can see that Occam's Razor has become a poleaxe: with these criteria, Baker and de Kanter found only 30 results acceptable out of the more than 300 reports in the current literature on transitional bilingual education. Studies could be eliminated if they asserted beneficial cognitive development or helpful ethno-social adjustment instead of increased English performance in English; studies could be rejected if there were "nonrandom assignment without control" (I, p. 11) for such factors as differences between oral and written language skills, or socioeconomic status, or ethnicity, or student's motivation and self concept, or positive and interested parental support, or varying

cognitive abilities--among others. In the norm-referenced criterion, studies were held invalid because in some cases minority students could learn English too rapidly and too well! (cf. I, p. 13). Baker and de Kanter seem ready to push beyond these standards when they can. A close examination of the studies held acceptable reveals that the authors are inclined to introduce new variables in order to hedge on the reasons for a program's success: thus Covey's programs (1973) may have worked because of a "low pupil-teacher ratio" (II, p. 5); if there is success through a concurrent translation method and a superior program design (the Legarreta (1979) study), then "good statistics are not enough" (II, p. 15); the Zirkel (1972) study made no allowance for differences in teaching excellence (II, p. 21), and so on. It becomes obvious that with this sort of intensive scrutiny, we have criticism of the third kind: neither partial, nor impartial, but instead counter-partial.

Reactions to the Baker-de Kanter analyses will be coming in for years, particularly from scholars whose work has been impugned, and whose experiments have been treated so cavalierly that they might never have existed. For now, it is with increasing suspicion and wonder that one ponders the method and the motivations behind the Baker-de Kanter report. Their analysis shows the same confusion between principle and implementation of principle which marred the AIR report: one cannot condemn transitional bilingual education because some practitioners have not yet gone about it correctly. The statistical overkill in the Baker-de Kanter report actually undermines the credibility of their research: it would be easier to accept the idea that something is inherently wrong with transitional bilingual education if there were, say, a 75% failure rate--but a 96.6% failure rate puts their criticism into the someone-doth-protest-too-much category. The very format of their analysis is suspicious: why, after a lengthy chapter treating the acceptable studies is there a special chapter devoted to a discussion of the inadequacies of just those studies which have been the pets, the favorites, of bilingual educational specialists in the past (Chapter III points out shortcomings in the Skutnabb-Kangas (1979) study, the Rosier-Holm (1980) Navajo research, certain of the Rudolph Troike (1978) analyses, and others)? What is behind this "Bring your barrel around so we may shoot your fish" attitude? The Baker and de Kanter report is thoroughly negative in tone, so that one perceives a thematic continuity in all the chapters. Oddly enough, the report is not paginated consecutively, while the individual chapters are. There is an apparent notion that each chapter can be pulled out and circulated by itself; any editor would judge that whatever purpose the entire report serves is certainly evident in any separate chapter. Above all, the report would have greater credibility if it had appeared during some other national administration. We have noticed that this particular method of analysis is suspect because the elimination-by-disqualification approach works best when there is a pre-established point of view which the researchers wish to protect in the first place. Thus when one reads the

conclusion that "the Federal Government should not place exclusive reliance on transitional bilingual education" (IV, p. 4) because in this report's analysis there was no evidence that "a specific program should be either legislated or preferred by the Federal Government" (IV, p. 4), one can only recall in dismay the statement on the first page that "this investigation was begun at the request of the White House Regulatory Analysis and Review Group" (I, p. 1). We must not lose sight of the fact the current administration has developed a policy which not merely temporizes, but obstructs, the Lau vs. Nichols decision of the Supreme Court. As reported in the Los Angeles Times (April 25, 1982), Mr. Daniel Oliver, general counsel of the Department Education, has chosen to implement the U.S. 9th Circuit Court of Appeals 1973 decision which preceded (and was then overturned by) the Supreme Court Lau vs. Nichols determination. It is obvious that someone will have to go to court to stop these obstructionist tactics, but in the meanwhile the administration may have bought time to orchestrate some new crescendo of anti-bilingual education opinion.⁵

In the final analysis, it becomes obvious that the critics of bilingual education are (to borrow a useful phrase from literary analysis) tempora-centric. The criticism of bilingual education (and thus bilingual education itself) is subordinate to the times, to politics, and to the availability of money (cf. Fishman 1970). When the time seems right, people will listen to Edwards, Epstein, Baker and de Kanter, and others who misreport the meaning of ethnicity, or to other spokesmen for sects within the Church of Happiness in Smooth Cash Flow. But when times change, people will listen to scholars like E. Glyn Lewis, James Cummins, and Joshua Fishman, who all along have had a deep and steadfast understanding of the real achievements and potential of bilingual education.

NOTES

¹Of course as Heinz Kloss has shown, minorities in this country have done this as well. See in particular his treatment of the American history of public schooling in German in The American Bilingual Tradition.

²In some cases there is re-identification. The 1970 and 1980 Americans censuses show that many individuals wish to reclaim a disused mother tongue (see for example Fishman's article on "Language Maintenance," pp. 629-638 in the Harvard Encyclopedia of American Ethnic Groups.)

³Notice here that Edwards has his classical sociology backwards. According to Durkheim, tyranny comes from the absence of mediating institutions, not from the presence of them (cf. Giddens 1972, p. 18).

⁴See Fishman 1978 for further reaction to the way the AIR report has been discredited; and compare Chapter 2, pages 58-60, of the Baker-de Kanter report (1981).

⁵Here is the sort of thing we may expect in the future: in Barnes (1981) the estimated population of school children who need bilingual education is lowered because there were "excessively high cutoff scores" on the original language proficiency test (p. 17). This means that educators formerly had unrealistically high expectations of what fluency in English was really like. For now, if we can accept a lower level of competence in English as the norm, then fewer students will need bilingual education. (And perhaps the Army can buy more tank ammunition.)

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NON-LINGUISTIC PARAMETERS OF INTERLANGUAGE PERFORMANCE:
EXPANDING THE RESEARCH PARADIGM

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Many of the claims made in the research literature on second language learning over the past decade have been predicated on data collected in large part through the implementation of some type of elicitation procedure. While some of the elicitation techniques utilized have been more structured than others, the intent of researchers has been to tease specific kinds of linguistic data out of respondents. This has been true for cross-sectional as well as for longitudinal studies. Although second language research has certainly uncovered important and interesting insights into the nature of the acquisition process, it does have some serious shortcomings, due mainly to its failure to take full account of the fact that interlanguages are as much natural languages as are native languages (Adjemian 1977).¹ One of the features of natural languages, as demonstrated by sociolinguistic research, is that they are subject to variation. Tarone (1979, 181) states the problem most eloquently when she writes: 'As second-language acquisition researchers, I believe we have not paid enough attention to one of the inherent characteristics of languages: in its use in human interaction, it varies with the subtlest shifts of situation, just as the chameleon changes color as its surroundings change.'

Second language researchers are not unaware of variation in interlanguage performance. Dickerson (1975), for example, following the research design used by Labov in some of his early work, found that second language learners do indeed vary their output according to the tasks they are asked to perform. LoCoco (1976) and Larsen-Freeman (1975), following a different research design, also discovered significant differences in learner performance as elicitation techniques were varied. In short, researchers have been able to uncover variation when they have specifically set out to find variation. The problem has been that for the vast majority of studies, unless their stated aim was an analysis of variation, they have assumed, implicitly at least, that their data reflects a more or less homogeneous, mono-stylistic interlanguage system. The data on which Schumann (1978), for instance, based his important study of the pidginization process was in fact collected in a variety of situations, including structured elicitation techniques, field trips to restaurants, museums, and parties, and spontaneous conversations between experimenter and subject; yet, as Tarone (1979, 187) properly points out, with the exception of the structured tasks, all of the divergently gathered data were collapsed into a single analysis.² McLaughlin (1980, 334) in commenting on the importance of Tarone (1979) remarks that unless researchers

pay attention to the variability present in experimental situations, the data simply cannot be interpreted with any degree of reliability.

The intent of the present study, is not to offer any new findings with regard to either acquisition orders or developmental patterns of interlanguage evolution, two major areas of second language research, but to provide what we consider to be clear evidence of systematic variation in interlanguage performance and to argue for expansion of the second language research paradigm in order to allow for interlanguage variability in the design of any research undertaken on second language, not just for those whose stated purpose is the uncovering of variation.

Burt and Dulay (1980) characterize the elicitation techniques used in most second language research according to task mode and task focus. Task mode refers to the type of language behavior undertaken by a respondent (i.e., writing, speaking, listening, reading). Task focus is determined by that aspect of the task to which a subject's attention is drawn (i.e., message conveyed or the linguistic forms used to convey the message). Task focus is bifurcated into natural communication and linguistic manipulation tasks. Manipulation tasks include such activities as discrete point grammar tests, editing activities, and the like. Natural communication techniques are further segmented into structured and unstructured tasks. Included among the structured communication tasks is the widely-used Bilingual Syntax Measure, which Burt and Dulay insist elicits natural conversation between subject and researcher. A question on the BSM such as 'Why do you think he's so fat?', asked by the examiner, while pointing to a fat character, according to Burt and Dulay, elicits an opinion or idea from the subject which is directed towards that specific situation, with the resulting speech generated with little focus on linguistic form (Burt and Dulay 1980, 300). While such an exchange may be viewed by children, the group with which the BSM was originally used, as a communication event, we doubt whether adults view such a task as natural communication. We suspect adults perceive the BSM technique as analogous to the well-known 'personalized questions' so often used in foreign language instruction. When the teacher asks a language student 'What did you have for breakfast today?' and the student responds 'Ham and eggs', there is certainly an exchange of semantic content, but the communicative value of the exchange is not very high. This is because the student generally does not believe that the teacher is truly interested in discovering his or her eating habits, but is essentially anticipating a linguistically well-formed response.

Unstructured communication tasks are such that researcher and subject engage in a natural conversation for which the investigator has no preconceived plans as to the specific structures he/she wishes to elicit. The advantage of structured tasks, for Burt and Dulay, is that they are more efficient, require shorter stretches of discourse and can more easily tease out the desired structures. Unstructured tasks, on the other hand, require a great deal more speech before the necessary structures appear with sufficient frequency for analysis.

Variation studies such as those already mentioned (i.e., Dickerson 1975, LoCoco 1976 and Larsen-Freeman 1975) as well as numerous others carried out by Krashen and his associates (see Krashen 1981 for discussion and interpretation), used elicitation techniques in which the participants were asked to perform tasks that were not necessarily reflective of normal language behavior. Dickerson's subjects were asked to read aloud from prefabricated dialogues as well as from word lists. In addition to writing compositions, LoCoco asked her subjects to translate passages and to describe pictures. Larsen-Freeman's respondents were given the BSM and were asked to complete a paper and pencil discrete point grammar task. Krashen's studies of adult second language acquisition likewise relied primarily on the BSM, in addition he utilized compositions, and the SLOPE test, developed by Ann Fathman. Tarone (1979) argues that picture description, the BSM, and SLOPE may not yield the same kind of variation found in real conversation. To be sure, Dickerson's study and at least one of Krashen's studies (Krashen, et al 1977) did use free conversation as a means of establishing a data base. The difficulty with these studies, however, is that they fail to describe the details of the situation from which the speech samples were extracted. That is, no mention is made of interlocutors (i. e., was there peer interaction only or was the interaction between experimenter and subject? Indeed, were the data even drawn from dialogues or were they taken from monologues in which the subject gives a type of oral report on a particular topic?); the topics are not known to the reader; the settings are not described (i. e., were the subjects speaking in a formal or informal context?). All of these factors, and others, have been shown by sociolinguistic research to impact on the linguistic behavior of a speaker. Moreover, with the possible exception of Dickerson's study, interlanguage research has failed to reveal any sort of continuum in style shifts. What has been shown is an all or none (i. e., monitored or unmonitored) alternation (Tarone 1979, 183). As sociolinguistic research has demonstrated, however, variation is a matter of degree of focus on speech and not simply a yes or no proposition. Consequently, without knowledge such as topic, setting, interlocutors, specific task, we cannot be certain whether a respondent is generating speech that indexes formal, informal, or vernacular styles.

It is our contention that what determines the communicative value of a task is not the task itself, as described by Burt and Dulay (1980), but the way in which the subject perceives the task. It is the respondent who assigns the communicative value, not the researcher. Thus, even if a task is seemingly unstructured, we cannot assume that a particular subject will perceive it as having a high degree of communicative value.

The data reported on in this paper are taken from a larger ongoing study of five students from the English Language Institute of the University of Delaware. Although we include partial data on all of the respondents, we intend to focus on only one of the five in detail, a male graduate student in civil engineering, whose native language is Arabic (henceforth referred to as M).

At the time of our initial interview, M had been in the United States for approximately ten months. He had studied English as a foreign language for five years in his homeland. On the basis of his TOEFL score, M had been placed in an advanced level English language class. Of the remaining subjects, two were also in advanced level classes and two were in beginning classes. Of the advanced students, one was an Arabic-speaking male and the other, a Spanish-speaking female. The same breakdown holds for the subjects in the beginning classes.

In this paper we will present an analysis of M's performance with regard to two aspects of his interlanguage morphology: nominal plural formation and article use. These forms have a high frequency of occurrence, thus facilitating the collection of a sufficient quantity of data for adequate analysis. We also include some interesting data on past tense marking, but past tense verb forms did not appear with sufficient frequency to permit a high degree of confidence in the statistics relating to this aspect of M's interlanguage behavior. In addition, we will present data on M's discourse performance, an aspect of his interlanguage behavior not unrelated to the morphological considerations to be examined.

The initial interview with each subject was conducted in the campus office of the principal investigator (henceforth L). Each interview lasted approximately one hour, although M's was slightly longer. All of the respondents were volunteers, and prior to the interviews were unknown to L. They were, however, acquaintances of the second investigator (henceforth K). The subjects were told that we would like to interview them for the purpose of studying their English language oral skills. Although both investigators were present during the initial interview sessions, L served as the principal interlocutor. Since our intent was to engage the subjects in an unstructured conversation, we had no preconceived strategy in mind when the interviews began. We felt that if we were to discover whether or not the topic, for example, could be shown to influence interlanguage performance, it would be best to allow the conversation to drift from topic to topic as might occur between monolingual interlocutors. At the outset, of course, L did have to establish the topics through questioning, in order to set the interaction into motion. It soon became apparent, however, that the subjects were not going to volunteer topics and L was forced to select all of the topics in order to sustain the interactions. The reasons for this will be discussed below. The interviews were taped, with the subjects' knowledge, and the recordings were then transcribed for analysis.

Table 1 displays the frequency of nominal plural markings for each subject in the initial interview.

Table 1 Short Plural Accuracy: Interview Task		
Subject	Count	Percent
O	43/45	96%
G	77/93	83%
M	42/79	53%
B	5/27	19%
R	2/17	12%

From Table 1 it can be seen that only 0 met criterion of 90% accuracy used by most second language researchers to score an item as acquired. Some investigators such as Schumann (1978) use a more liberal 80% criterion level. In this case G would also be scored as having acquired the short plural.³ The counts for B and R are low, because they had a difficult time sustaining a conversation and tended to respond with brief one or two word utterances. They both displayed a high degree of topic avoidance and message abandonment, generally regarded as signs that a learner has not acquired sufficient linguistic competence in a second language to cope with the task at hand.

As pointed out above, the interviews had no preordained topics. The topics ranged from attitudes toward American food and life-style, dating, travel experiences, cultural contrasts, and the like. Near the conclusion of the interview with M, the topic of Ramadan, the month of fasting for Moslems, was introduced by L. It seemed like an appropriate subject, since the interview was being held during this season. Gradually, the topic of conversation shifted to a discussion of the similarities and differences between Islam and Christianity, a subject of intense interest for M. When the interview concluded, M expressed a desire to return in order to explore the topic in greater depth. This was the first time any of the subjects had even attempted to initiate a topic. As it turned out, M returned the following week for what amounted to a total of about four hours of conversation focused exclusively on a comparison between the two religions.

In order to distinguish between the two unstructured tasks, we refer to the initial task as an interview and the second task as a conversation.

A comparison of M's performance on the two tasks, reveals a significant difference in accuracy of plural marking.⁴ It will be recalled from Table 1 that M had scored 53% on the short plural on the interview task. On the conversation task, however, he scored 30/84 or 36%. Following the conversation task, we asked M to participate in the BSM with L as the examiner. This time his performance on short plurals improved in accuracy to 96% or 27/28. Table 2 compares M's performance on all three tasks: the structured BSM, the unstructured interview and the unstructured conversation.

Table 2
 χ^2 Analysis of Short Plural by Task for M

BSM	Task		χ^2
	Interview	Conversation	
27/28(96%)	42/79(53%)	30/84(36%)	
x	x	x	31.132*
--	x	x	5.021**

*p < .001, **p < .025.

It seems clear that x for M plural marking varied not only as a function of task but of topic as well. On the structured task, M's performance met criterion for acquisition, but in neither of the unstructured tasks did his marking of the short plural approach the requisit 90% level. The analysis in Table 2 then indicates a clear stylistic continuum in his interlanguage behavior with the BSM representing the most formal style and the conversation task the least formal.

As in the case of the short plural, M's use of articles was also subject to variation across tasks and topics. Although most second language studies, with the notable exception of Huebner (1979), do not distinguish between the acquisition of definite and indefinite articles, treating both as a single morpheme, we have found a clear difference in M's use of the two articles, as can be seen in Table 3.

Table 3
 χ^2 Analysis of Article Use by Task for M

Feature	Task			χ^2
	BSM	Interview	Conversation	
The	5/12(42%)	41/59(69%)	45/98(46%)	
a. --		x	x	8.185*
b. x		x	x	9.270**
A(n)	12/14(86%)	4/22(18%)	6/50(12%)	
c. --		x	x	.491
d. x		x	x	32.066***
Total	17/26(65%)	45/81(55.5%)	51/148(34.5%)	
e. --		x	x	9.573#
f. x		x	x	15.144***

*.005 $< p < .01$

** $p < .01$

*** $p < .001$

#.001 $< p < .005$

If we consider the two morphemes as a single form (line f of Table 3), as has been done in much of the literature, we observe a significant difference in M's performance across the three tasks. As in the case of the short plural, M's accuracy increases as we progress from unstructured conversation, to unstructured interview to the BSM. This analysis, however, obfuscates what is really happening in M's article usage. When we segregate the articles into definite and indefinite forms, we discover a much more complicated state-of-affairs. Disregarding the BSM data for the moment, we note that M's performance with regard to the indefinite article was consistently low across the two unstructured

tasks (line c) and that significant variation occurred in the case of the definite article only (line a). The apparent anomaly that shows up when the BSM data is included in the analysis (lines b and d) must, for the time being, remain unexplained.⁵

Finally, as mentioned earlier, we do have some data on M's use of past tense marking on the unstructured tasks. Although the numbers are not sufficient to permit reliable statistical analysis, we still observe the same type of variation across tasks as noted for pluralization and article selection (see Table 4).

Table 4
Past Tense Marking by Task for M

Feature	Task	
	Interview	Conversation
Past Irregular	8/12(66%)	7/23(30.4%)
Past Regular	4/ 4(100%)	7/18(38.6%)

We now turn our attention beyond morphology, to the domain of discourse. As will be demonstrated, not only did M vary his linguistic behavior in terms of morphology but he also presented clear evidence of variation in his discourse patterns across tasks.

It is difficult to imagine that the BSM task was for M anything more than an exercise in linguistic form. While there was semantic content conveyed on this task, the communicative value of what was transmitted was from M's perspective, shallow. Despite the claims that have been made about the capacity of the BSM to stimulate natural communication, the evidence from M's performance is clearly contradictory. We discovered that M consistently repaired his output, especially with regard to plural formation. For example, in responding to one of the pictures on the BSM, he said, 'There are cloud in the sky' and quickly changed to 'There are clouds'. According to Krashen, et al (1977, 340), monitoring, especially repair, is an indication that a speaker is attending to form. Moreover, throughout the task, M frequently punctuated his utterances with the phrase 'It's enough here?'. He was asking whether he had generated enough output to satisfy the researcher. On the basis of this kind of evidence, we must conclude that for M the task did not represent natural communication but was simply seen as an exercise in linguistic manipulation.

Turning now to the unstructured tasks, it would seem that we have two instances of natural discourse; yet, we have uncovered significant differences in M's performance on the two unstructured activities. While both tasks were unstructured, and according to Burt and Dulay should have induced natural conversation from our respondent, the interview task did not, from M's viewpoint have the communicative depth of the conversation task. This we attribute in large part to a difference in topic. In a sense, M had a vested interest in the topic of the conversation (i. e. religion), whereas, the interview represented for him little more than the kind of information exchange that might occur at a cocktail party or on a job interview. An analysis of the discourse generated by

M supports our claim.

Although M's behavior on the interview task was nowhere near as monitored as on the BSM, he did, nevertheless, exhibit signs that he was attending to form. On several occasions he altered nouns for plurality (e.g. 'tall building' to 'tall buildings' when speaking about a recent visit to New York City). Perhaps the most significant indicator of M's attitude toward the interview was his comment made near the conclusion of the session when L was called away from his office for a few minutes. At this point, M quickly turned to K and remarked, 'Do you think it's enough to make me speak? It's your turn.' Again, as in the case of the BSM, M was asking whether he had produced enough speech to satisfy the investigators; clearly, not a comment made by someone truly interested in participating in a meaningful conversational exchange.

There are additional factors in M's discourse behavior that lead us to the same conclusion. The mean length of utterance (MLU) for M on the interview task was 3.91, whereas on the conversation task, his MLU increased to 6.83. According to Giles and Powesland (1975, 122), research has shown that utterance duration increases when speakers engage in a conversation on a topic of special interest to them. A further and perhaps even more convincing indication of the importance of topic saliency is what we have labelled as mean length of turn (MLT). This is a measure akin to MLU, but which takes into account the average amount of discourse produced by a speaker on a given turn throughout an entire conversation. On the interview task, the MLT for M was computed at 7.583 words, with a range of from 1 to 32 words per turn. On the conversation task, on the other hand, M's MLT was calculated at 16.09, with a range of from 1 to 129 words per turn. If, as the evidence seems to indicate, M was attending more closely to form in the interview task than in the conversation, it is not surprising that both his MLU and MLT should be lower on the former than on the latter task. As Selinker and Lamendella (1981, 210) maintain, monitored speech is a more costly operation in terms of energy concentration required. Hence, it would be consistent with this position to find a learner producing more limited and constrained utterances in the monitored mode than when opting out of this mode.

Although topic saliency and task seem to have influenced M's interlanguage behavior, the situation may be even more complex than it at first appears. We believe that another, until now overlooked factor, must be considered when conducting second language research, and that is the interactional or role relationship between interlocutors, including the subject/researcher dyad. The role relationship between interlocutors is not a factor which is completely separate from the task and topic, but is closely allied with them.

The research of Robert J. Di Pietro (1976, 1981a and b) has drawn our attention to the need for linguistic analysis to look beyond its traditional concern with what he calls the artifact of language (i. e. its structure). Di Pietro believes that in addi-

tion to the artifactual dimension of language use, there are two other dimensions which qualify as legitimate domains of linguistic research: the transactional dimension (i.e., the use of verbal tactics and strategies to achieve a goal) and the interactional dimension (i.e., the use of language in accordance with the roles played by the participants of a conversational dyad).

According to Pit Corder (1981, 103) how we communicate is 'determined not only by our knowledge of the language, but also by our current assessment of our interlocutor's linguistic competence and his knowledge of the topic of discourse.'

Di Pietro (1976, 463) argues, however, that consideration of context, speaker intent, and shared information is in the domain of artifact and does not take account of the two other dimensions of a conversation. Clearly, there is no reason to assume that the three dimensions of language use proposed by Di Pietro are any less relevant in a subject/researcher dyad than they are in other speech situations.

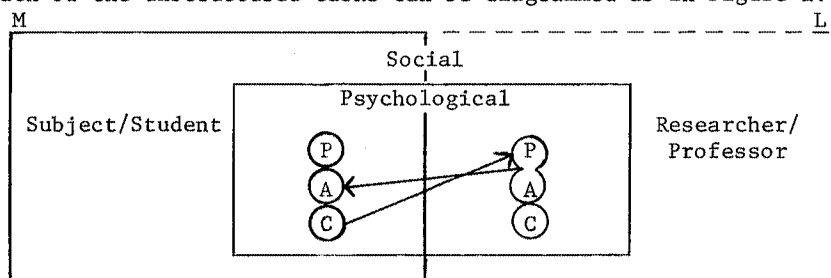
According to Di Pietro (1981b, 472), the interactional dimension of conversation is the most complex and difficult to deal with, because roles may be complementary or overlapping and may even shift throughout the course of a conversation. To date, Di Pietro has discussed three role categories: social (e.g. buyer/seller), emotive (e.g. rivals, friends) and maturational (e.g. adult, parent, child). The task for the linguist is to uncover the verbal characteristics ascribed to particular role types (Di Pietro 1981b, 472). Our concern here is with the effects of the interactional dimension of the unstructured tasks on M's linguistic behavior.

To begin, we propose a modification of Di Pietro's motive and maturational role categories. Both of these role types stem from the psychological side of man and thus can be combined into a single psychological role category, which we refer to as ego states, following the psychological model proposed by Eric Berne (1964). According to Berne, all individuals develop three ego states during their formative years. These ego states, referred to as Child, Parent, and Adult, remain with us throughout life. At any given time any of the three states can be actualized and take control of our interaction with the external world (Berne 1964, 2). Each ego state can be identified by the particular behavioral patterns associated with it, including linguistic behavior. The Adult ego, for example, manifests itself through such verbal phenomena as the asking of questions and expressions like 'probably' and 'in my opinion' (Stevick 1976, 69). The Parent ego, on the other hand, reveals itself linguistically through expressions like 'always', 'never', 'should', 'ought', and 'if I were you . . .' (Stevick 1976, 67). An indication that the Child is in control can be seen in such phrases as 'I wish' and 'I dunno' (Stevick 1976, 68).

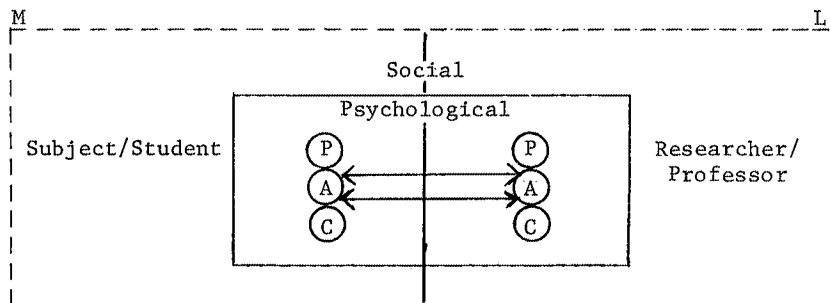
In human interactions, role relationships may be complementary, in which both members of a conversational dyad agree on the roles played by themselves and the other member of the dyad. That is, one member may play the Child to the other's Parent, or both may agree to play the Child. In such cases, communication will

flow smoothly (Berne 1964, 30). Roles may also cross, however, in which case one member of the dyad may manifest an ego state that conflicts with the state displayed by the other member. For instance, if one interlocutor manifests the Adult and attempts to address the other's Adult while at the same time the second interlocutor tries to play the Child to the other's Parent, the result is a crossed interaction which usually makes communication more difficult (Berne 1964, 30).⁶ It is important to keep in mind that what on the surface appears to be an Adult/Adult interaction (i.e. a conversation between two adults) may in fact be a Parent/Child interaction or even a crossed interaction, depending on the perspective adopted by each interlocutor.

In our opinion, the role relationships between M and L for each of the unstructured tasks can be diagrammed as in Figure 1.



A. Interview Task



B. Conversation Task

Fig. 1. Role relationships on unstructured tasks

In Fig. 1, A represents a crossed or at least partially crossed interaction. M perceived the interaction as between Parent/Child ego states, while for L, the relationship was essentially between Adult egos. The open circles on L's side of the diagram indicate that the boundaries between his Parent and Adult states were not completely closed; hence, showing that some bleeding took place between these two states. What induced M to play Child to L's Parent was in all probability due to the prominence he assigned to the social roles of each interlocutor. L's social role as researcher/professor was perceived by M to be dominant to his role as subject/student. M's perception of the

relative social status of each interlocutor was manifested by his adoption of a psychological Child role which he played to L's Parent. L, on the other hand, did not assign prominence to the social status of the interlocutors, symbolized by the broken line enclosing his side of the diagram. He therefore undertook to interact with M's Adult ego state from the perspective of his own Adult state. M's relative success at maintaining the interaction as Parent/Child, however, forced L to partially cathect, Berne's terminology, his Parent state in order to sustain the interaction. The same phenomenon occurred in the interviews with the two beginning level respondents, B and R. In this case, however, L's cathecting of his Parent state was unable to sustain the exchange to the extent that it did with M, because both B and R had an extremely limited interlanguage, which may even be characterized as presystemic, to use the terminology of Selinker and Lamendella (1981).

B of Fig. 1 characterizes the situation once the topic shifted exclusively to religion. The importance of the social roles was now backgrounded for both interlocutors, as indicated by the extension of the broken line to include M's portion of the dyad. This backgrounding, in our opinion, was triggered by M's ardent interest in the topic of conversation. Thus, the interaction between M and L, took on from M's perspective, greater communicative depth. The optimal means for him to manifest his interest in the topic was to activate his Adult ego state and simultaneously anticipate an Adult response from L (see Berne 1964, 29). The decathecting of L's Parent is readily understood, since Adult/Adult was the interaction he had sought from the outset.

Some of the linguistic consequences of the social and psychological roles played by the interlocutors have already been touched upon in the preceding discussion. We argued, for example, that the MLU and MLT of M's performance on the two tasks was influenced by topic selection. This claim, however, requires some refinement in light of our analysis of the interactional dimension associated with each task. While topic is usually a function of role relationships between individuals, especially for those marked in Di Pietro's terms as [+episodic], for M, the situation was reversed; i.e., topic served as the principal catalyst of change in his perception of the social and psychological dimensions of his interaction with L. This change in role relationships, in turn, impacted on M's linguistic performance.

In analyzing the data from the interview task, we did not find a single instance of nominal embedding in M's utterances. This, of course, may be traceable to his native language, since Arabic tends to favor coordination over subordination. The implementation of transference strategy is not uncommon in cases of monitored interlanguage performance (Tarone 1979). M's avoidance of subordination taken together with the abbreviated duration of his utterances is parallel to the findings of a study on school-aged children reported on by Giles and Powesland (1975), in which it was found that children in the sixth grade tended to use a simplified register, shortened utterances, and simplified syntax when interacting with their teachers (a Parent/Child interaction). Giles and Powesland (1975) conclude that the children were re-

acting to the power status of the teacher by adopting a laconic speech style, which served as a self-defense mechanism and expressed an unwillingness to enter into an extended dialogue. A similar strategy was also displayed in M's performance on the interview task, as illustrated by his comment at the conclusion of the session to the effect that he felt he had spoken enough accompanied by his request that K take the floor. Furthermore, at the outset of the interview, M made a series of comments that were clearly intended as a defensive ploy (strategy according to Di Pietro's analysis). Upon being questioned about his English language classes, M responded as follows: 'I don't like to, to learn English ... I don't like languages in general ... I don't like Arabic ... I like to talk, but when I have to.' As Forman and Ramsburg (1978) point out, defense is the domain of the Child ego state.

In the conversation task, the picture changed considerably with regard to embedding. M generated a total of 29 utterances containing at least one embedding each in NP position. Again combining this data with the increased duration of his utterances as measured by MLU and MLT, we find that his performance on the second task approximated the speech style employed during peer interactions by the same group of school children discussed by Giles and Powesland. Furthermore, in the conversation task, M did not make a single statement that could be construed as emanating from a defensive posture, as he did on the first task.

During the interview task, M did not ask any questions, usually a manifestation of the Adult ego state (Stevick 1976, 69). L, on the other hand, asked forty-three questions, representing slightly more than half of his total utterance production. Of the forty-three questions posed by L, thirty were yes/no questions, requiring a simple answer from M, and only thirteen were wh-questions. Typical of L's questioning, however, was his tendency to provide a possible response to his own inquiries. One of the questions posed by L, for example, was 'Why did you go to Niagara Falls?', which was followed immediately by a possible answer 'You went to Niagara Falls just to see the falls?' inserted before M had the opportunity to respond. Frequently, L even recast what began as an information question into a yes/no format, as in the following example: 'What's the one thing that really . . . is there anything that you haven't gotten used to or really bothers you about, that you find very difficult to deal with in American culture?', to which M responded: 'No'. The only information questions which L did not rephrase were those that required a simple numerical response, as in 'How long have you been in the U.S.?' The questioning strategy employed by L in the interview task was similar to the simplification strategy often adopted by parents when interacting with their children.⁷

In the conversation task, the questioning pattern changed markedly. M asked sixty-seven questions, including fourteen information questions, thirty yes/no questions, and twenty-three requests for L to elaborate on or clarify a point. In a discussion of the rules on fasting in the two religions, M remarked:

'You mean, ah, you mean you have to fast during Monday until Sunday?'. During the conversation, L asked only five wh-questions and eleven yes/no questions, all of which occurred in the early stages of the interaction.

Finally, we turn briefly to evidence regarding topic shift. Of the twenty-six shifts in topic that occurred during the interview task, all were suggested by L. In the conversation task, however, of a total of eight topic shifts, all relating to the general theme of religion, six were initiated by M and only two were proposed by L. Clearly, an indication that the role relationship between the interlocutors had changed.

To summarize: the overall communication strategy adopted by M in the first interaction can be characterized as risk avoiding, or as referred to by Pit Corder (1981, 105) message adjustment strategy--a strategy marked by topic avoidance, message abandonment, and message reduction. M's strategy in the second interaction was clearly risk taking, or as labelled by Pit Corder, resource expansion strategy. Among the features of resource expansion are borrowing, circumlocution, paraphrase, and appeals for help from an interlocutor. Risk avoiding strategies emanate from a defensive posture in which a speaker sacrifices message content in order to avoid generating linguistically ill-formed structures. From M's viewpoint such a strategy is understandable, if indeed, as we have claimed, the social parameters of the interview situation were paramount, thus inducing him to play Child to L's Parent.

In a sense, M's primary concern was not to convey information, but to comply with the Parent/Researcher's request for a sampling of his interlanguage. None of the topics suggested by L nor any other factors for that matter were of sufficient impact to alter M's basic perception of the interactional relationship between the interlocutors.

In the conversation task, because of his intense interest in the topic, M was willing to take whatever risks necessary in order to discover and convey as much information as possible. To achieve this M had to background the social roles of the interlocutors, therefore freeing himself to adopt an Adult/Adult perspective on the interaction. Recall that it is in the Adult/Adult mode that communication between individuals is most effectively carried out. Once M adopted the Adult/Adult perspective, he no longer had to 'play it safe' linguistically speaking, and could afford to relax any concern he might have had concerning his linguistic abilities. In fact, at several points throughout the conversation he even resorted to a high-risk tactic of making a point through the use of complicated analogies. In explaining his understanding of 'faith', he used the following analogy: 'but for example, we know there is, there is in the wire of the electricity, there is a wire but we know there is inside the wire there is electricity, can you tell me there is no electricity inside the wire? No, you see the light but you didn't see the electricity, right? But there is something led you to say there is something cause this to exist, right?'

Conclusions: our findings appear to have important implications for second language research. The interlanguage performance of learners is not simply a function of whether a task is labelled as communicative or manipulative or whether it is structured or unstructured. The typology of tasks discussed by Burt and Dulay is based on a set of assumptions that do not take full account of the learner's perspective of a task. We not only uncovered clear evidence of variation across structured and unstructured tasks, but we also found significant levels of variation across two unstructured tasks. The evidence confirms Tarone's hypothesis that as in the case of first language use, style shift in interlanguage performance is a matter of degree and not simply a question of monitored versus unmonitored speech. Even more importantly, perhaps, we have presented evidence that there is another dimension of language use, beside artifact, that the second language researcher can no longer ignore. The role relationships (social as well as psychological) between interlocutors was shown to have a significant impact on the interlanguage behavior of a speaker. While topic was seen to be a factor which can influence role relationships between interlocutors, surely, there are factors (e.g. physical setting, cultural background of interlocutors, etc.) that will come to light as we pursue research along the lines of the expanded paradigm suggested in this paper.

It seems clear that second language researchers must exercise caution when making claims about interlanguage performance and what this tells us about a learner's development toward the target language. One cannot assume, for instance, that a respondent has acquired a given structure simply because he/she has met criterion on a particular task, e. g., the BSM. Such an assumption, however, has been the rule for second language studies, especially for those researchers who engage in large-scale cross-sectional studies in which, for practical considerations, only a single elicitation technique can be used. We believe that because of the non-linguistic parameters of linguistic performance more detailed individual case studies, such as exemplified by the work of Wong-Fillmore, must be undertaken by second language researchers before complete understanding of the processes involved in the acquisition of other languages can be developed. An intriguing question raised by the present study and which has not been addressed by second language research is does the acquisition hierarchy proposed by Burt and Dulay and Krashen and his associates apply to all speech styles used by an interlanguage speaker? Krashen's research implies that the answer to the question is yes, but he has not taken account of the factors that we have shown to play a role in interlanguage useage, nor has he considered interlanguage performance in other than monitored/unmonitored modes. Also to be resolved is the issue of whether interlanguage variation reflects alternations between old and new infrasystems, as proposed by Selinker and Lamendella (1981) or whether it mirrors a single underlying competence with variable rules, as suggested by Dickerson (1975). We can begin to answer these questions only if we expand the present research paradigm in order to take full account of the relationship between the linguistic and the non-linguistic aspects of interlanguage behavior.

Notes

¹In studies on child second language acquisition, in particular, there have been several studies based exclusively or in large part on non-elicited speech samples (see Hakuta 1974, Kessler and Idar 1977, and Wong-Fillmore 1976). Wode et al (1978) have studied the interlanguage of adolescents and adults using non-elicited data. Schumann (1978) also employed natural data in his study of Alberto, but there are some difficulties with his analysis to be discussed below.

²Since his study was longitudinal, Schumann did necessarily consider variation due to the time factor.

³We include only the short plural -s in our data, since the long form -es did not occur with sufficient frequency to permit analysis.

⁴For a full discussion of article development in a learner's interlanguage the reader is referred to Huebner (1979).

⁵Berne uses the term transaction when referring to role relationships between individuals. For linguistic reasons, we prefer Di Pietro's analysis of conversation and therefore adopt his terminology.

⁶It may be the case that 'foreigner talk', in general a simplification strategy used by natives when interacting with non-native speakers of their language, is a reflection of the Parent ego state. The possibility is certainly worth further investigation.

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AN EXPERIMENTAL REAPPRAISAL OF SOME SYNTACTIC CONSTRAINTS ON CODE-SWITCHING

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1. INTRODUCTION. Language change such as creolization or pidginization, which can be induced by languages in contact, has long been the object of intensive study by linguists, but the phenomenon of code-switching resulting from interlingual impact and characterized by an interplay of two languages in the natural flow of conversation, on the contrary, has received far less attention to date (cf. Pfaff 1976, p. 248). As a fairly common communication phenomenon, code-switching is important not only because, as Shaffer (1975) contends, it "affords a glimpse into the process of sentence formation" (p. 492), but also because it invariably carries subtle psycholinguistic and socio-cultural implications (cf. Gumperz and Hernandez-Chavez 1972, p. 98).

Although the term code-switching is used with several different meanings in the literature, in the study reported here it refers to the alternation of two (or sometimes more) languages within a sentence. Here is an example of such code-switching; it occurred in the home of a Chinese family residing in Edmonton, Canada, and involves English, Taiwanese (known to both mother and child), and Mandarin (known just to the mother).

Child:	ma-ma, góa beh aī	/	juice
	'Mother, I want		juice'
	Taiwanese		English
Mother:	juice	/	khng tī
	'Juice		is in
	English		Taiwanese
		/	bīng-syāng ²
			the refrigerator'
			Mandarin
Child:	peng-siu ⁿ	/	not
	'[Say] peng-siu ⁿ		not
	Taiwanese		English
		/	bīng-syāng
			bīng-syāng'
			Mandarin

This little dialogue is of interest not only as an example of the phenomenon under consideration, but also as an illustration of several other points. The first is that, while most of the code-switching examples discussed in the literature have involved related languages, e.g. Spanish and English, or Russian and French, the behaviour is not limited to such pairs but is a universal phenomenon induced by languages in contact, and should be explainable in universal terms. A second point is that even though the sentences of this example of code-switching in natural conversation are fairly short, they are equally as perplexing epistemologically as longer examples reported elsewhere (Hasselmo 1970, Lance, 1975, Redlinger 1976, Valdes Fallis 1976). Lastly,

the adult participant in the dialogue, the mother, in response to questions about her code-switching, was conscious of reasons for some of the switches, but could not account for others. She explained that she had said juice rather than Taiwanese kô-chiap or Mandarin gwo-jĩ because juice implied the North American reconstituted frozen concentrate in her refrigerator, whereas the Chinese words implied fresh fruit juiced in a blender. Such a shift seems ostensibly a function of the semantic domain of the topic--a kind of situational appropriateness. The informant could not explain, however, why she had switched to Mandarin (bĩng-syāng) rather than using either English refrigerator or Taiwanese peng-siuⁿ. Obviously this was neither prompted by any compelling lexical constraints, nor by well-understood situational motives (cf. Weinreich 1953, p. 48; Haugen, 1953, p. 380). The only explanation available for now is that "frequency and recency of encounter and use" may be the occasion of this lexical shift (cf. Shaffer 1975, p. 493), though this is not empirically verified, and hence not a completely satisfactory answer. Such evidence as this example, which is paralleled by many other similar cases reported in the literature, suggests that the reasons that code-shifts occur, and the constraints upon where they occur, are neither simple, nor open to explanation by introspection.

What does seem to be clear, from the personal experience of the first author and from many cases of self-report, is that there is a set of psycholinguistic and socio-cultural constraints that are operative in implementing code-shifts such that, when confronted, informants may be able to make reliable acceptability judgements of code-switched sentences (cf. Gingras 1974), and even sometimes to offer illuminating comments on various mechanisms that trigger switches. This assumption provides the basis for the experimental methodology of our study.

2. THE RESEARCH QUESTION. The hypothesis that language users do not code-switch randomly--that this is rule-governed behaviour--has been accepted by many researchers (Clyne 1967, Blom and Gumperz 1972, Gingras 1974, Timm 1975, Lipski 1977, and Wentz 1977). However, the basis for, and nature of, the rules which govern where a code switch may appropriately occur in a sentence are less well agreed upon. The aim of the present study was to investigate further, in an experimental framework, some of the syntactic and socio-cultural constraints on intrasentential code-switching.

Clyne (1967, p. 343) observed that switching between two languages may be conditioned by either internal or external factors. In externally conditioned switching a shift is said to be activated as a result of such sociolinguistic factors as the speakers assessment of "the others' ethnic identity, age, sex, degree of solidarity or confidentiality," and of the setting of the conversation and the topic (cf. Gumperz and Hernandez-Chavez 1972, p. 98). In describing internally conditioned switching, Clyne seems to be concerned with linguistic causal factors, especially "trigger words" such as cognates, proper nouns, and established loanwords" (1967, p. 345). Other researchers such as

Lance (1969), and Pfaff (1976) have also suggested that the rules governing code-switching are based on lexico-cultural factors. Pfaff, for example, says that "intrasentential code-switching is most basically a social and cultural phenomenon (1976, p. 258)."

Timm (1975), however, postulated that the constraints were syntactic in nature, and showed that there are at least five surface structure constraints on Spanish-English code-switching which, she argued, "help preserve the syntactic integrity of each language." These constraints are: (1) against switching between pronominal subjects or objects and the finite verbs to which they belong; (2) against switching between finite verbs and their infinitive complements; (3) against switching between auxiliaries and the main verbs; (4) against switching between a negative element and the verb negated; (5) limited switching in NPs containing a determiner and an adjective. Timm further speculated that these syntactic constraints might be applicable to other language pairs, saying that "cursory perusal of examples" seemed to reinforce her findings (p. 481).

The basic hypotheses tested in the present study are, first, that the surface-structure constraints on Spanish-English code-switching are to be accepted as part of a universal grammar as Timm contended, and should thus be borne out as well in a Chinese-English code-switching task. Consequently, the study sought to subject five surface-structure constraints to further empirical test. Second, the hypothesis is tested that the governing constraints on, and causes of, intrasentential code-switching are a combination of both syntactic and lexico-cultural considerations. The study attempts to illuminate the functioning of the two factors.

3. METHOD. The test battery for the study consists of twenty-five simple code-switched sentences, with the switch occurring at the point which Timm (1975) showed was not acceptable. Each of Timm's syntactic constraints was illustrated by five sentences (one block), with some modification in one block in the light of language-specific factors. At least one sentence in each block was restricted to strictly syntactic testing; that is, the lexical items involved were neutral with regard to cultural factors, and the sentence was predicted to "sound wrong" to a fluent code-switcher on the basis of syntactic disruption alone. Culturally marked or language specific factors were introduced into the remaining sentences in each block. These factors were lexical items having referents strongly associated with either Chinese or English culture (e.g., using chopsticks), or items which--although having referents common to both cultures--are much more semantically complex in one culture or the other (e.g., elder brother). Such an arrangement is witnessed by the block of sentences reflecting the surface-structure condition against switching between a negative element and the verb negated:

I don't	/	chřdàu	'I don't know'
tāmen bú	/	come	'they don't come'
you don't	/	chřu	'you don't go'
wǒ bù	/	skate	'I don't skate'
they don't	/	yǜng kwàitz	'they don't use chopsticks'

The first three sentences are lexically neutral, and re-tested Timm's syntactic constraint. The last two use verbs such as yǜng kwàitz 'use chopsticks' and skate, which are lexico-culturally specific.

Three of the blocks (III, IV, V) follow this pattern. Two of the blocks differed slightly. Block I, testing Timm's constraint against switching between pronoun and verb did not allow for the substitution of a "culturally-marked" pronoun. Consequently, the test in this case involved substituting for the pronoun either a common or a proper noun, in each case culturally marked, to test Clyne's (1967) claim that a proper noun is of particular importance in "internally-conditioned" switching. The nouns were Richard, an English proper name, Jāng Shan, which fills the same role in Chinese that John Doe does in English, and gēge 'elder brother' which is semantically much more complex in Chinese than in English.

The block based on Timm's verb and complement constraint, Block II, differs from the main pattern because of language specific factors. In English the complementizer to is required after the verb want (I want to read). In Chinese, no complementizer is required: wǒmen yāu lái is literally 'We want come'. The test, in this case, involved comparing sentences with and without the complementizer present, and included some other constraints which will be discussed in the section on results.

Eleven of the stimulus sentences began in English and switched to Chinese, and fourteen began in Chinese and switched to English. At least two of each sort occurred in each block. All twenty-five stimulus sentences appear in Table 1.

The stimulus sentences were randomized for presentation to subjects. The subjects, who were tested individually, were asked to rate each sentence for acceptability on a four point scale. The four possible judgements were characterized as good, all right, funny, and very funny. In the analysis of responses, these four categories were reduced to two: accept or reject. Sentences judged good or all right were classified as accepted; those judged funny or very funny as rejected. Provisions were also made to elicit from the subjects provocative comments about the justification of their acceptability judgements.

Two different subject groups were tested. One consisted of seven native speakers of English who had taken intensive modern Standard Chinese for at least two years at the University of Alberta. The other consisted of sixteen native speakers of Chinese who had finished their college education in Taiwan and had lived and worked or studied in North America for at least two years prior to the testing. Although most of the subjects are not "balanced bilinguals" in a technical sense, they are functionally fluent in code-switching, and when interviewed before testing,

Table 1
Chinese-English Code-Switching Test Materials
and Percent of Acceptability Judgement

I. Pron + V

1. you / chīfàn	'you eat'	(0%;0%)*
2. nǐ / go	'you go'	(0%;0%)
3. gēge / go	'the older brother goes'	(88%;100%)
4. Richard / chīfàn	'Richard eats'	(100%;100%)
5. Jāng Shān / go	'John Doe goes'	(100%;100%)

II. V + Comp

6. they want to / lái	'they want to come'	(0%;57%)
7. wǒmen yào / read	'we want to read'	(75%;29%)
8. they want / lái	'they want to come'	(5%;0%)
9. wǒmen yào / to read	'we want to read'	(0%;29%)
10. wǒmen yào / reading	'we want to read'	(38%;0%)

III. Aux + V

11. he can / pǎu	'he can run'	(0%;0%)
12. wǒ hwèi / skate	'I can skate'	(94%;86%)
13. he can / yòng kwàitǎ	'he can use chopsticks'	(25%;29%)
14. wǒ hwèi / skating	'I can skate'	(50%;0%)
15. wǒ hwèi / to skate	'I can to skate'	(0%;0%)

IV. Neg + V

16. I don't / chīdǎu	'I don't know'	(0%;0%)
17. tāmen bú / come	'they don't come'	(12%;0%)
18. you don't / chū	'you don't go'	(0%;0%)
19. wǒ bù / skate	'I don't skate'	(63%;0%)
20. they don't / yòng kwàitǎ	'they don't use chopsticks'	(19%;29%)

V. (Poss) Adj + N

21. her beautiful / shū	'her beautiful book'	(0%;0%)
22. nǐde pǒ / clothes	'your tattered clothes'	(0%;0%)
23. his favorite / tsài	'his favorite dish'	(21%;100%)
24. wǒde / hot dog	'my hot dog'	(100%;100%)
25. wǒde / steak	'my steak'	(88%;100%)

*The first score is the percentage of native Chinese speakers and the second the percentage of native English speakers accepting the sentence as good or all right.

they indicated that code-switching is a rather common practice among them.

4. RESULTS AND DISCUSSION. Table 1 presents, for each test, the percentage of each subject group, Chinese native speakers (C-group) and English native speakers (E-group), who judged that sentence acceptable (i.e., either good or all right). In Table 1, the first set of sentences in each block (except II, to which the distinction does not apply) tests Timm's syntactic constraint on code-switching using culturally neutral lexical items; the second set introduces culturally "marked" words. The scores for the culturally neutral sentences (1, 2, 11, 16-18, 21, 22) show that an overwhelming majority of subjects in both the C-group and the E-group rejects these sentences which code-switch at the syntactic boundaries interdicted by Timm's Spanish-English results. The only sentence of this type accepted at all was 17, which a very small number of C-group speakers judged acceptable. This finding adds weight to Timm's claim for universality for her switching constraints. However, it is clear that the picture changes drastically when culturally-specific lexical items are present in the stimuli. Of sentences containing such items (3-4, 12-15, 19, 20, 23-25) only one, 15, is rejected by both groups, and that presumably because it contravenes both Chinese and English structural rules. The acceptance of sentences of this second, culturally "marked" type, in which a syntactically-prohibited switch is over-ridden by lexico-cultural considerations, provides a rather dramatic demonstration both of the complexity of the constraints and compulsions that govern switching, and of the dominance of semantic factors over syntactic ones.

It may be of some interest to comment on each block of sentences individually. In Block I, in which switching between subject pronoun and verb was disallowed, the percentage of acceptance increased dramatically when the pronominal subject was replaced by either a language specific proper noun such as Richard (100%;100%) or Jāng Shān (100%;100%), or by a culturally-marked common noun such as gēge 'the older brother' (88%;100%). This dramatic rise in acceptability demonstrates that the Pron + V constructions were not rejected because the stimuli are too short and simple and the code-changes are too abruptly made in the sentences (cf. Wentz 1977, p. 158), but rather because "the choice of lexical items which are code-switched is not entirely free" (Wentz 1977, p. 148), and indeed is dictated by socio-cultural and internal factors in the sense of (Clyne 1967, Lance 1969, Gumperz and Hernandez-Chavez 1972, and Pfaff 1976). Furthermore it is observed that there is a certain class of words that are not generally code-switched such as grammatical function elements, i.e., pronouns, and very simple common nouns such as man (Gingras 1974) which can be termed a set of semantically less complex category. Wentz (1977) found that semantically more complex adjectives had a higher degree of acceptability for switching among his informants (p. 150). Consequently it may be hypothesized that intrinsic semantic complexity, which encompasses socio-culturally marked features, may in fact play a crucial role

code-switching.

This pattern of the rejection of culturally neutral sentences which contravene Timm's syntactic constraints changing to acceptance when culturally specific lexical items are introduced, is also observed in Blocks III (Aux + V), IV (Neg + V), and V (Adj + N), with slight variations in the responses of the two subject groups to the presence of certain lexical items which were more strongly culturally marked for one than for the other. Block V provides an interesting example of this. Subjects in both groups almost unanimously responded that switching in sentences 24 and 25, which switch to English hot dog and steak respectively, is perfectly acceptable. All of the subjects in both groups almost unanimously responded that the switchings made in wode/hot dog 'my hot dog' (100%;100%) and wode/steak 'my steak' (88%;100%) are perfectly acceptable. For the Chinese subjects who had disallowed the switch of sentence 25, wode/steak, the Sinicized nyóupái 'steak' would have been preferable because, as several explained, that term evokes a perceptually relevant image in the same way as does the indigenous term páigú 'spare rib'. This was in contrast to sentence 24, wode/hot dog, in which the use of the Chinese term règǒu (rè 'hot', gǒu 'dog'), literally 'a dog which is hot', would have provoked a ridiculously misleading image.

This harmonious picture of acceptance by both the C-group and the E-group begins to deform when a lexical item is used which is assessed differently by the two groups in regards to its cultural identity or specificity. For instance, sentence 23, his favorite/tsài 'his favorite dish (vegetable)' was judged as perfectly acceptable to the E-group (100%), but much less so by the C-group (21%). One E-group subject remarked that tsài implies "Chinese cuisine" to him, therefore, he accepted the switch. However tsài to the Chinese subjects is but a simple common noun.

An interesting footnote to this point about lexico-cultural specificity, and the judgement of it by the two subject groups, may be seen in sentences 13 and 20, which contain the lexical item yòng kwáitz 'use chopsticks'--presumably a concept strongly linked to Chinese culture. Note that the two subject groups are relatively in agreement in their judgements, (25%;29%) for sentence 13 and (19%;29%) for sentence 20, and that the acceptance level is fairly low compared, for example, to sentences 24 and 25 or sentence 12 in which the culture-marked item is skate. Apparently the use of chopsticks is no longer quite as exclusive to Chinese culture as it once was seen to be.

Two other points of interest emerge from Blocks III and IV. First, if we compare sentences 12 and 19, we see that 12 conforms to the pattern we have seen elsewhere in that the presence of a culturally marked verb (skate) makes a presumably syntactically-prohibited switch (cf. sentence 11) highly acceptable. However, skate does not have the same effect in sentence 19, which is completely rejected by the E-group and accepted by only 63% (as compared to 94% for sentence 12) of the C-group. This suggests that the syntactic prohibition against a switch between Aux and V is not equivalent to that between Neg and V. Further, note that

in English the Neg is carried on an Aux, whereas in Chinese it is a particle (bù). This would account for the difference in evaluation of sentence 19 by the two groups; the E-group's refusal to accept it at all reflects the fact that the switch is actually made at a point interdicted by two syntactic constraints, the combined weight of which apparently cannot be overcome by a culturally-marked word.

The second interesting note concerns the C-group subjects' evaluation of sentence 14. (E-subjects rejected it completely since skating is an inappropriate form in English with any translation of the Aux used.) C-subjects however, reflect in their 50% acceptance a dichotomous view of the nature of form hwèi. Li and Thompson (1981) view it as an Aux, in which case the sentence would be rejected since the English skating is inappropriate to follow an Aux. Chao (1968), however, views it as a main verb ('to be capable of') for which skating could act as an appropriate object.

In regard to Block II, the two subject groups showed an interesting variation in responses to the constraint on V + Comp switching. The C-subjects (0%) resoundingly rejected switching between an English finite verb and its infinitive complement such as they want to/lái 'they want to come' (sentence 6) whereas the E-subjects partially (57%) accepted it. A similar code-switched sentence with modified V + Comp pattern, *they want/lái (sentence 8) was rejected outright by both groups (5%;0%) however. The problem with *they want/lái, of course, is the complementizer to which is missing in an essentially English sentence. Structurally paralleled with this sentence is Timm's problematic sentence *they want a venir 'they want to come.' Wentz explained that the complementizer a in this instance should be "in the code of the element which determines its use or non-use," according to Lipski (1977), the portion of a code-switched sentence that goes before the point of shift may be syntactically divergent, but the portions appearing after the switching must be essentially identical syntactically (p. 271). That is, in an essentially Chinese sentence (7), wǒmen yào/read, the English complementizer to is not needed. Apparently the Chinese subjects processed *wǒmen yào/read as a Chinese sentence, therefore they accepted (75%) it, since read meets the syntactic requirement of the Chinese verbal complement; but on the other hand, only a handful of E-subjects (29%) accepted it. The variation of responses between C- and E-subjects to sentence 9, wǒmen yào/to read (0%;29%) indicated that the two subject groups may process the same sentence differently--in other words, a few Canadian subjects obviously interpreted this sentence as an English one which requires the complementizer to to identify read as an infinitive, and thus ignored Lipski's syntactic-copying hypotheses mentioned above. Sentence 10, the last in this group, was rejected by the E-subjects since reading is an inappropriate complement form in English for the verb yào 'want', but accepted by many of the C-subjects since in Chinese yào may take an object, for which reading is appropriate, as well as a complement.

Any study should, as well as answering specified questions, propose yet other, new questions to investigate. The study reported here has shown that constraints on intrasentential code-switching are both syntactic and lexico-cultural in nature, and that of these two the latter appears to take precedence. However, the picture cannot be said to be clear, and one of the most interesting remaining questions could be approached by using a larger group of subjects and a more extensive set of stimulus sentences to determine the interaction: group X language with which the sentence begins X type of switching (culturally neutral vs. "marked"). It seems that such a multi-variate approach may be required to unravel the complexities governing this most interesting behaviour.

Notes

1. In memory of the first author's mother, Mrs. C. C. Lai Lin, who initiated him into code-switching in his childhood days at the dinner table. The authors would also like to thank Professor S. R. Munro, Chairman of the Department of East Asian Languages and Literatures at the University of Alberta, for his inspiring suggestions and insightful comments. Of course, all frailties are ours.
2. About romanization of Taiwanese and Mandarin Chinese, see Cheng, R. and Cheng, S. Phonological Structure and Romanization of Taiwanese Hokkien. Taipei: Student Book Co., Ltd., 1977. Fenn and Tewksbury, Speak Mandarin. New Haven: Yale University, 1967.

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"FOREIGNER TALK" IN SOCIAL INTERACTION

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1. INTRODUCTION

The object of this paper is to apply an analytical distinction between "formal" and "informal" speech varieties to English-language "foreigner talk" (FT) so as to isolate and elucidate certain essential cultural dimensions in the use and thus the definition of informal FT. FT consists in a native speaker's (NS) use of a simplified register for spoken and written communication with non-native speakers (NNS), and there is a point of contact between the study of it and of the acquisition of a second language in the literature on "teacher talk" (TT) in classrooms where English is taught as a second language. Krashen (1980), drawing on the empirical studies of Henzl (1974), Gaies (1977), and others, emphasizes the instructional value of TT, FT, and "baby talk" (BT), of which the latter is the original member of Ferguson's expanded family of "simplified registers" (Ferguson 1981), and we shall have more to say on this subject of instructional value below.

A paper presented to the Seventh LACUS Forum in 1980 (McCormack 1981) emphasized clarification as the central function of foreigner-talk discourse (FTD) and as the feature that differentiates FT from BT within the family of simplified registers. That paper was based on the FT used by a native-English-speaking adult female passive bilingual in Lithuanian to speak and write to her Lithuanian-American mother, who speaks and writes to her by means of broken English interlanguage (BL). Freed's recently completed doctoral dissertation has reached the same conclusion about the importance of clarification (Freed 1980, 1981), though, while my data have been rich in utterances formed "just to get the job of messaging done with whatever aids are at hand" (McCormack 1981: 516), Freed's subjects are said to have produced only well-formed, grammatical, utterances (Freed 1980: 20). The question of grammaticality in simplification remains unresolved (Clyne 1981a: 6; see also Lattey MS.), but since "simplification is rarely if ever the exclusive diagnostic characteristic of a particular language/

variety/register in comparison with another" (Ferguson 1981: 12), the door remains open also for research into the social interactional, cultural, diachronic, and psycholinguistic dimensions of FT.

One social interactional feature that turns up in FTD, both in my data (which continue to be taken from the same FT speaker as before), and also in data recently reported by Day (MS.) for NS/NNS conversations in Hawaii, is a significantly different structure for conversational repair than had been reported by Schegloff, Jefferson, and Sacks (1977) for NS/NS discourse. According to the latter authors, speakers should initiate self-repairs, and hearers should not carry out other-repairs. However, as Day (MS.) has shown, FTD between friends seems to replace the rule dispreferring other-correction with rules relating to social interactional expressions of community of reference group between self and other. We discuss below how cases of ostensible self-correction by our FT speaker are actually instances of one kind of modulated other-correction that proceeds from the BL speaker's and the FT speaker's momentary psychological unity.

Another social interactional feature of FTD has topic-initiation in spoken English discourse proceeding by means of statements in NS/NS interaction but by means of questions in NS/NNS interaction (Long 1981). Gaies (MS.) has questioned the construct "FT register," but at the same time has provided supporting experimental evidence for Long's contention that questions are preferred in FTD because they coerce participation from NNS.

The point of the present paper, however, is to stress that a full and sensitive interpretation of FTD requires attention not only to social interactional framework but also to the FT speaker's expressive culture because that significantly affects the communicational functioning of FTD within the given social interactional framework. Speaker's expressive culture has been analytically neglected in foreigner-talk research. Ferguson has long postulated a cultural origin for simplified registers, speaking of "the conventionality or cultural givenness of a [simplified] register which is transmitted from generation to generation," and recently reiterating his 1975 position that "written material, especially literary use of FT to represent the speech of a foreigner," should be consulted in research (1981: 13). Freed disagrees with him fundamentally, possibly because her own data base was limited to the culturally deprived spoken FT of casually

acquainted college students in contrived settings. I share Ferguson's view of the value of written text (McCormack 1981: 518) since it seems to me that simplifications/clarifications in written FTD probably bespeak a planning of discourse (cf. Ochs 1979) or at least some advance knowledge about trouble spots to be avoided in communicating with BL addressees. If this is so, it would qualify, or limit the generality of, Freed's claim that "self repetitions in FT were found to be motivated by an expression of misunderstanding on the part of the foreigner" (Freed 1981: 31). Consideration of written FTD should reveal some cultural reasons for the considerable individual variability in FT (Hatch et al. 1978; Clyne 1981b: 78), while also underscoring the continuing validity of Ferguson's 1975 observation that, "Even those NSs with little prior foreigner talk experience apparently have access to most of these [simplifying/clarifying] resources" (Long 1981: 136), and I freely include some written text below.

Here, the first order of business is to discover analytic criteria for distinguishing "formal" from "informal" varieties of FT, and Ferguson has usefully pointed to the literature on language acquisition by stating that, "The two most active areas of FT research at the present time [include one]...in relation to the study of language acquisition" (Ferguson 1981: 13).

Krashen speaks directly from that concern in hypothesizing (1980: 8) that:

'Foreigner talk' may be of two kinds... native speakers' imitations of second language speech, or rather, their acquisition of aspects of their interlanguage...after prolonged contact with second language speakers.... The [other] sort of foreigner talk...is the simplified input native speakers may give to less than fully competent speakers....

Of his two kinds of FT, Krashen himself does not elucidate the first kind, perhaps because he perceives it to be not one of the "simple codes...of tremendous help to [language] acquirers at early and intermediate stages, child and adult, first and second languages" (Krashen 1980: 16) that are of primary interest to him. For us, on the other hand, it is the first kind which is most characteristic of our FT speaker, which is in effect a code for clarification, and which thus serves as a convenient starting point for our own present effort to define an "informal" FT.

As for provisional definition of a "formal" variety of FT, that too gets an assist from Krashen, in collaboration with Seliger (1975). Though not being yet aware of the significance of simplified registers for research on language acquisition, Krashen and Seliger produced a virtual componential analysis of the pragmatic domain of the TESL classroom, and concluded that the essential contributions of formal instruction to adults learning a second language reduced to the following two characteristics of formal speech events:

1. Focus, in which the TESL teaching isolates rules and lexical items, and
2. Monitored social interaction situation, in which the possibility of error detection or correction is provided by the classroom.

For the time being, I take these distinctions as diagnostic of "formal" FT, and note that they are absent from the texts on which the present study is based.

Though it is convenient to operationalize the above criteria to meet my coding problem here, that is not to run them into the ground. Pragmatically speaking, from the standpoint of his own professional concerns, Krashen (1976) has shown for oral proficiency in English that TESL classrooms function as both formal and informal linguistic environments. Given Krashen's distinctions, though, we can now turn to analyzing a case study of FTD to arrive at a more refined definition of "informal FT." We shall see that my subject's use of FT is confined to use of informal FT, and that her informal FT not only produces clarifications but also signals the existence, or attempted creation or re-creation, of a communicational context of intimacy. It is that kind of context which enables certain specific conversational processes that are peculiar to informal FT and that we summarize under "Discussion and Conclusions."

2. TEXTUAL ANALYSIS

If our FT speaker is primarily a speaker of informal FT, then her BL/FT interaction might begin as a formal speech event but would be expected to transform to informal as a result of her FT's exerting contextual pressure away from attention to rules and toward topical diffuseness. Our FT speaker feels that her BL interactants often "relax" into diffuseness during her BL/FT interactions, and it is in fact evident in a se-

quence of events in a tape-recorded conversation, analyzed below, between her and two BL addressees. This text resulted when our FT speaker, about to tour Lithuania for the first time, placed a long-distance telephone call to her father's sister's son (BL-B) and his wife (BL-A), both of whom eventually became simultaneous parties to the call by means of extension phones in their home. These relatives are Lithuanian-American adults who immigrated to the United States about 1949, who speak Lithuanian between themselves, and whose English is BL.

The content of the conversation may be summarized as follows:

FT speaker phones BL-B and his wife BL-A to ask where her father had grown up in Lithuania. BL-A explains how things went on her own recent trip to Lithuania, and, from both BL speakers, FT speaker gets some information about her father's place of origin, the fact that all his siblings are now dead, and that his village also is now gone. FT speaker also hears about BL-A's medical difficulties and about the BL speakers' children.

This summary was written by our FT speaker after she listened to the tape, and it reveals her retrospective interpretation of the occasion as formal in that she foregrounds a "focus" on eliciting information about Lithuania. BL-A's medical difficulties, and the news of her children, are placed at the end of the summary, although chronologically in the conversation they occur at the beginning. A lexical marker of the formality of the occasion appears when the FT speaker takes leave with the words, "Well listen, I got to go, my friends ..."(at #462); she might, of course, have said something like, "Sleep well, [sound of] kiss kiss, m'love," which happens also to be entirely within her range of expression.

FT speaker opens the conversation with BL-A in a quasi-ritualistic and precise telephone-talk style, using her normal intonation rather than her FT intonation, and establishing her identity. Thus the opening sequence in this conversation with her male cousin's wife, which is unlike her opening in phone calls to her mother, is:

BL-A: Hallaa?
FT: Hello...Alex?

BL-A: Aa?
FT: Is this Alex?
BL-A: Yes.
FT: Hello...this...is...An-na...

There then begins FTD, just after personal recognition is achieved by BL-A's normal-English exclamation (one that should not elicit FTD, according to Freed's analysis):

BL-A: O my goodness!

Thus, our FT speaker now accomplishes a second opening that is intonationally and lexically similar to phone calls to her mother:

FT: Helloo sweetie lady how are you
Alex...

In the following sequence, BL-A narrates her recent medical history, and in the course of that she self-repairs (at #55) as follows:

BL-A: I don't have nothing aa what you call
...like sharp pain...or nothing.

Now (at #98), FT speaker explains the occasion for the phone call, and frames the interaction using her FT intonation and timing/rhythm associated with that intonation:

FT: Listen...the main reason I'm phoning
I know you're a little surprised to
hear from me because we don't usually
telephone
BL-A: [Responds irrelevantly]
FT: But listen...the main reason I'm
phoning...
BL-A: uh huh
FT: ...is because I'm going to Lithuania.

FTD is now well established, and BL-A's relaxed diffuseness along with it. So BL-A monologues on, and persists in not hearing FT's repeated question, "Where did you meet your relatives?" Finally, after much co-speaking, the question is heard and gets answered (at #150) in Lithuanian:

BL-A: I Vilnius! ["In Vilnius"!]

Since BL-A much later (at #475) expresses surprise that FT speaker understands Lithuanian, BL-A could not have

been aware at the earlier point that she had answered the question which FT speaker persevered in asking until an answer was supplied by BL-A.

From this example I infer that use of informal FT can render a BL/FT interaction frame so topically diffuse that the FT speaker has to request confirmation repeatedly (= ostensible self-correction) by way of steering the conversation back (= modulated other-correction) from the irrelevancies, or side sequences, nominated by BL-A.

Another instance of this same process (at #207) even contains the suggestion that a BL speaker responding to informal FT can have her attention to English syntactic rules drop momentarily completely out of sight:

- FT: Well you mean there is no way...then
 ...even if I was able...allowed you
 know...to go...
 BL-A: oh no no
 FT: ...there's no way I can see my fa-
 ther's place...
 BL-A: No...nothing is it...just a...just a
 ...ground.

A written-FTD example of this process of collaborative self-/other-correction by our FT speaker appears in a letter to her mother (dt. July 27, 1982):

The other good news in your letter was that you heard from IM--that's absolutely wonderful to know they are well--good! I had sent her a name-day card for her name day on--oh no, her name day isn't until September 18th and it was ZM I sent a card to for his name day on June 23rd. But I was late with it, and they will perhaps be late sending me a name-day card. They usually do send me one, for the same date they send one to you. Anyway, the main thing is that some one of us has heard from them, and that they are well.

3. DISCUSSION AND CONCLUSIONS

Though the data presented in this paper strengthen the impression I share with Clyne and Hatch that there exist many individual varieties of FT register, we have argued that at present it is a step forward in analytic classification within FT research to conveniently adopt

Krashen's (1980) criteria for distinguishing "formal" from "informal" varieties of FT and then to move ahead, via case study involving expressive culture, to refine the definition of informal FT. Thus, from this new perspective, the TT isolated by Henzl (1974, 1979) could be viewed as a variety of formal FT, analogous to the distinctive varieties of learner language (formal BL) which Krashen's testing procedures found were "learned through formal pedagogic means" in ESL classrooms (Ferguson and DeBose 1977: 118). Similarly the English FT elicited by Freed from conversations between college students would class as a formal code, because of the monitoring implicit in the academic milieu of the interviews and because of situational monitoring attendant on conversations between strangers. Clarification there may be in both cases, but Freed's FTD data are not the same in kind as those produced in social interactions by our FT speaker after her "prolonged contact with [the] second language speakers" (Krashen 1980: 8).

At the same time, to examine informal FT as a separate functional variety of FT register is to discover an essential psychological condition underlying the production of informal FTD, namely, the FT speaker's empathy for the other conversational partner (Hatch et al. 1978; Katz 1981). In communication, this empathy operationalizes into a process of apparently reading the other's mind (Tarone 1980: 424) and then "saying for" him (for a similar process in BT, see Goffman 1979). In other words, the FT speaker acts as a "crutch" for the BL speaker, steering the conversation by culturally sensitive *qua* collaborative self- and other-corrections (and by what is not studied here, topic avoidance; see Tarone 1981: 290) which, since "empathy" involves a "fusion of self-object boundaries" (Guioro et al. 1975: 44) and thus renders the conversational partners momentarily psychologically one, enable the BL speaker to subside and/or confine to a deteriorated performance of formal language. We leave aside here any question of the role of empathy in a FT speaker's L₂ acquisition, which is more debateable (Schumann 1978: 296).

The clarification processes operative in informal FT are not in any direct sense language-teaching devices, and Krashen's observation about the pedagogical irrelevance of informal FTD is correct as far as it goes. However, it would be interesting if also methodologically difficult to compare the L₁ of BL speakers who have received informal FTD with that of those who have not, for FTD-inspired innovation in the BL speak-

ers' language-evidenced perceptions (Tarone 1981: 291; see Haugen 1956:79 and 1969:72 for the causal effects of immigrants' second culture on creating a "mixed" variety of their L_1). It may be that in the area of cultural learning informal FT is indeed "instructive," and that the informal FT speaker serves efficiently--though unconsciously--as an agent of the BL speaker's perceptual/linguistic acculturation. If so, informal FT would have to be added to other forces (cf. Martinaitis 1977; Reklaitis 1977) converging to give rise to a BL speaker's mixed variety of "American Lithuanian."

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THE INFLUENCE OF FRENCH ON ENGLISH IN QUEBEC

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One scholar has stated that "translating consists in producing in the receptor language the closest natural equivalent to the message of the source language, first in meaning and secondly in style"(1). Thus translation is an example of language contact. Translators are perfectly or near bilingual (or multilingual) and they work in two or more languages every day. Language contact also occurs in a bilingual population like that of Canada, in which people are confronted with the same reality, expressed differently in each language. From this contact arise interferences between the different languages or codes, that is, "the use of elements of one language [...] while speaking or writing another"(2). In translation, the degree of interference is generally minimal since by its very nature, it is a self-conscious and self-critical process. Outside this context, however, interference is likely to be high and results in ambiguity, confusion in the message, even misinterpretation. This applies not only to the spoken idiom, which lies outside the purview of this article, but also to the written medium: the press. One such example of interference through language contact is the occurrence of gallicisms.

The number of books on anglicisms in French Canadian is legion and the work of Colpron, Darbelnet, Dulong, Mackey and others on the subject is too well known to need discussion here. On the other hand, little or no attention has been given to the influence that French may be having on English. This article is an attempt to fill that gap.

Basically, a gallicism can be defined as a word, an expression or a grammatical construction borrowed from the French language. In the province of Quebec, the number of gallicisms infiltrating English usage appears to be rising. By English usage is meant the way that language is spoken in Quebec as opposed to other provinces or regions. Being recent newcomers to Quebec, we are in a privileged position to judge what is Quebec English, although this does not exclude the possibility that certain "gallicisms" may be found in forms of English usage characteristic of other parts of anglophone Canada or the United States.

The perspective of this analysis of gallicisms is synchronic. Hence, the data presented here is drawn from

current English-language newspapers in Quebec: the *Montreal Gazette* and the *Quebec Chronicle Telegraph*. These were chosen as being representative of English usage in those areas from which they originate. As will become obvious from the examples presented, there is a considerable difference in the standard of written English.

Sampling was carried out in two stages: first in 1979 and again in 1982. It involved sifting through the *Gazette* and *Chronicle Telegraph* and noting down on file cards examples of English usage that appeared to result from the influence of French as spoken in Quebec. The examples were then checked and placed in two general groups: definite gallicisms (those which required minimal verification) and possible gallicisms (or those that required extensive verification).

The gallicisms listed as possible were then further analyzed. First, dictionaries and grammars were used to determine whether the English word, expression or construction believed to be influenced by French exists or ever existed in English with the identical meaning; then TEFL specialists and colleagues outside the province were consulted to ascertain whether words, expressions and constructions whose meanings correspond to those recorded in the dictionaries and grammars were not actually gallicisms in terms of current usage.

International dictionaries and grammars (*Webster's Third*, *Oxford English*, *Gage*, *Leech and Svartik*, *Frank*, *de Bruyn*, etc.) were consulted to ensure that apparent gallicisms did not in fact correspond to the varieties of English spoken in Britain, the United States and English Canada. Possible archaisms were verified in etymological dictionaries and historical grammars. Care was taken not to repeat the error Colpron made in his book on anglicisms, by verifying in the *Petit Robert* whether the French word thought to lead to the gallicism is indeed acceptable current usage in that language. Thus, a possible gallicism was rejected if it had one of the meanings recorded in the dictionary or if the source from which it seemed to originate was not acceptable French usage.

Dictionaries and grammars cannot define all instances of legitimate usage in English. Thus, specialists in the field were consulted to help clarify items that were correct English but unidiomatic to the native ear. This process made it possible to reduce the number of possible gallicisms. Those items on which a consensus was not reached remained in the possible group.

Both groups of gallicisms, definite and possible, were

then further divided into categories adapted from the work of Darbelnet and Colpron in order to fit the examples gathered during sampling. These categories, of which there are three, are as follows: lexical, syntagmatic and syntactic, each of which was further subdivided into definite and possible gallicisms. Various types of definite and possible gallicisms occur in each category.

Lexical items

By lexical items is meant individual words and proper nouns in contrast to syntagmatic and syntactic structures. They include definite and possible gallicisms which subdivide in the following categories:

1. Definite gallicisms

- a) Full borrowings: *tabagie* (newsstand a good example of modulation in English), *carnaval*, *caribou*, *dépanneur* (corner store), *subvention* (grant);
- b) Full or partial deceptive cognates (*faux-amis*):
to accede to (to be appointed to), *to affirm* (to declare), *to ameliorate* (to improve), *animator* (leader), *to apply* (to enforce), *assistance* (care), *attached to* (connected with), *attendance* (audience); *composing*, intr. (*constituting, making up*), *comprehension* (understanding), *compressions* (cutbacks), *conference* (talk), *confronted* (in opposition - a double *faux-sens* since this error is based on the anglicism "confronter" for "affronter" in French, *course* (class), *create* (make - occurs in the expression "to create a small dent in..."); *delay* (period), *dimension* (aspect), *discourse* (speech), *disputed* (fought), *dynamic*, (*driving force*); *to encourage* (foster - occurs in the expression "to encourage a spirit of sharing", *to escalate*, intr. (*to increase*), *eventual* (possible, likely), *evolution* (development), *expenses* (expenditure), *to experiment* (to experience); *fabricated* (made), *to favour* (to promote), *to foresee* (to plan); *to group* (to contain); *interior*, adj. (*inside*), *intentions* (presents); *named* (nominated); *presentations* (speeches); *raffled* (carried off i.e. won); *sack* (bag), *to set* (to devise - occurs in the expression "to set a joint emergency plan"), *solicit*, vb. (*request, apply for*), *spectacle* (show); *to tell* (to say), *touched* (affected); *varied* (various), *voice* (vote), *welcoming* (hospitable).
- c) Proper nouns: *Montreal Urban Community* (Metropolitan Montreal), *La Brasserie Labatt Limitée* (Labatt

Breweries Ltd), the Rouge et Or (Laval University), the Old Capital (Quebec City), the French Language Office (the Office de la langue française).

- d) Spelling: *généalogie* (genealogy), *Parti Québécois* (*Parti Québécois*), *préambule* (preamble), *responsabilité* (responsibility), *story* (storey), *victime* (victim).

2. Possible gallicisms

- a) Archaisms: *ignore* (not to know).
- b) Dictionary definition vs. Usage (i.e. those words that according to the dictionaries correspond to the meaning in the context, but which seem unidiomatic to the native speaker): *accept to* (*agree to*), *adolescent* (*teenager*), *drivers permit* (*drivers licence*), *juridical* (*legal*), *meteorological forecast* (*weather forecast*), *reimbursement* (*refund*), *school commission* (*school board*), *Metropolis* (*Montreal*).
- c) Erroneous usage, I.e., use of words that distort meaning: *raindrops* (*showers*), *snowflakes* (*snowflurries*).
- d) Frequency I.e., word usage typical of Quebec as opposed to other areas of English-speaking Canada: *pharmacy* (*drugstore* - this term not being synonymous with its US homonym which excludes idea of place where prescriptive drugs are sold).

Syntagmatic items

These designate collocations of lexical items, in which one of the items is a literal translation of the corresponding French expression: *classification test* (*placement test*), *demand for payment* (*request for payment*), *opening hours* (*business hours*), *physical conditioning* (*physical fitness*), *services not rendered* (*services not provided*).

Syntactic items

The term "syntactic" is used in its broadest sense to designate grammatical or structural elements.

1. *Definite gallicisms* - these include items transferred directly from the French: *in the best of traditions* (*in the*

best of traditions), in the case where (whenever), solicit the employment (apply for employment), mass of thanksgiving (thanksgiving mass), choice place (preferred place, location), at grips with (in the grips of), a call to my shop (a call at my shop), a place of rally (a rallying place), provide openings into other careers (provide openings in other careers), its right to veto against the bill (its right to veto the bill), take an early retirement (retire early) within hospital personnel (among hospital staff), not advertised items (non-/un-advertised items), they must conform with the ruling (...comply with), that, that's a little insulting (that's [tonic stress in spoken English] a little insulting), they should rather learn French (rather, they should learn French), the JF Hospital provides services cheaper than other bigger hospitals (...provides cheaper services than...), they were 118 to receive their diploma (118 received diplomas), a battle lost involved English and French speaking parents (one lost battle involved...), there would be no changes brought to the budget (...no changes in the budget), as a build-up to the career day the whole school had participated in... (...to career day, the school participated...), the boys pee-wee team have yet the series to play (...still have the series...), the court has no hesitation to conclude (...in concluding), the program will be highlighted when Mr H will become... (when Mr H becomes...), the program wasn't late coming (...was introduced promptly).

2. Possible gallicisms - these generally involve confusion of structures i.e. French structures can generally be translated in more than one way in English, depending on idiom and meaning. Following are instances where the writer has made a wrong choice: *ethical code* (code of ethics), *persons of the Golden Age* (Golden Age people, Golden Agers), *a well-merited vacation* (a well-deserved vacation), *a commemorating plaque* (a commemorative plaque), *no manoeuvring margin* (no margin for manoeuvre), *the program of the First Year* (the First-Year program), *educational priorities for the development process* (...developmental process), *AR died suddenly in his home* (...at his home), *it had to be expected*. *Claude Morin had no other choice...* (it was to be expected/inevitable. *Claude Morin had no other...*).

The *Chronicle Telegraph* appears more prone to the various types of gallicisms just described. This, of course, would tend to suggest that its reporters are francophone or near francophone, a fact that must be considered in further studies. The *Gazette*, on the other hand, is much freer of French influence, and the gallicisms noted have tended to be of the subtler variety (e.g. *The French Language Office*, *take an early retirement*). Such a situation can best

be explained demographically. The English-speaking population in Montreal is concentrated in one area and maintains close ties with the rest of English Canada. The anglophones living in Quebec City are a small, scattered community, isolated from the Montreal and English Canada.

In conclusion, it should be pointed out that the foregoing is a pilot study and treated as such. Any further research in the area must entail a much larger corpus of examples, as well as extension of the investigation into other written sources such as translation and literature. Further revision and modification of categories used to classify gallicisms may also be in order.

NOTES

1. Nida, p. 23
2. Mackey, p. 195

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VIII

HISTORICAL STUDIES

SYLLABIC WRITING AND THE DISCOVERY OF VERB-ROOTS

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We have grown so accustomed to the linguistic term *root* that we scarcely think of it as an import from the medieval Semitic grammarians. Hebrew grammars, written in Latin in the sixteenth century, were the first to apply this technical sense to *radix*, literally translating {šōreš},¹ which in turn had owed its technical meaning to the Arabic {ʿaṣl} (literally 'root' or 'trunk').² But *radix*, *Wurzel*, *racine*, *root* was rarely extended to the description of European languages till late in the eighteenth century, when Sanskrit was brought to bear upon them. The Indian grammarians, as early as Pāṇini in the pre-Christian era, had a different term {dhātū}, whose primary meaning (outside of linguistics) was 'a foundation' (or possibly 'a layer'). The European Orientalists, beginning with Nathaniel Halhed,³ equated it with root from their Semitic studies. They went on to find in Greek, Latin, etc., whenever possible, the cognate to each Sanskrit {dhātū} or (as they called it) root.

My purpose is not to retrace the semantic history of a term that is now altogether familiar, but to ponder what characteristics of the several languages themselves, and of the grammarians' methods, made the roots perceptible much earlier in some languages than in others. For there is a lesson from which we linguists near the end of the twentieth century can profit or take warning.

The events in the world at large that have established the Latin alphabet as the main international script on every continent have a counterpart in the restricted realm of linguistics: there too an essentially Latin alphabet of vowels and consonants, though with various modifications for phonetic accuracy, serves as the basis for scientific notation of every language and for any subsequent analysis.⁴ So far as one can see, there is no practical alternative, neither within the community of linguists nor in the world of commerce, technology, and diplomacy. Even if there were, we should never belittle the advantages of such an alphabet -- the greatest of them being that it is easier to learn than any other script ever devised.⁵ Nevertheless, as theoreticians, the linguists ought to balance the merits and the limitations, and among the latter is something which lies behind the historical fact that roots were not discovered in any languages of Europe during many centuries when these were written in the Greek, Latin, or Cyrillic alphabet. The vowel and consonant letters did not make the roots evident and may even have obscured them.

The Brahmin grammarians were the first to pick out what modern Occidental grammar calls the root, from which a multitude of

actual words are formed by suffixing, prefixing, and internal changes.⁶ For example, they identified रिच् {ric} 'leave' as the {dhātu} or root from which are formed the present रिणमि {riṇāgmi} 'I leave,' the perfect रिरेच {rirēca} 'I have left' or 'he has left,' रिसिचथुः {riricāthuh} 'you (dual) have left,' the aorist आरैक् {ārāik} and अरिक् {ārikta} 'he left,' etc. How they succeeded in thus abstracting a root is unclear from the meager history of scholarship in ancient India. They seem not to have been influenced much -- perhaps not at all -- by Nāgarī or other related scripts that were at least beginning to be used in India. For they were primarily priests, conserving old oral texts and orally analyzing the language of those texts and of their caste. The {dhātu} was an abstraction to show how the divergent actual words are related to one another and share a certain meaning. The processes called {guṇa} and {vrddhi} were mentioned in Pāṇini's opening words and bespeak an exact recognition of how such forms as {rirēca} and {ārāik} differ from {riricāthuh}, {ārikta} internally, apart from any prefix or suffix, and what all three have in common. The alternation of {i} with {ē} (actually a diphthong [ei]) and with {ā} was analyzed so as to make {ric}, with the least open vowel, basic.

The ancient Greek and Latin grammarians did not abstract λιπ- and liqu- respectively from λεῖπε, λιπε ("present" and aorist imperative singular), λέλοιπα 'I have left,' λοιπός 'left over,' and from linque (imperative singular), liquē 'I (have) left,' *relictus*, *reliquus* 'left behind,' etc. Varro, about 45 B.C., did occasionally use the term *radix*, but in a somewhat different sense, as a word from which other words arise. The most pertinent passage goes thus: "Words like *legō* 'I gather,' *scribō* 'I write,' *stō* 'I stand,' *sedeō* 'I sit,' etc., which are not from any other word but have their own roots, are called primitive. Derived words are those which arise from some other, as *legis* 'you gather,' *legit* 'he gathers,' *legam* 'I will gather' from *legō*, and thus very many from this same one."⁷ The second sentence shows that Varro has no clear conception of *leg-* as the root of *legō*; for if he had, he would not derive *legis*, *legit*, *legam* from *legō*.⁸

Subsequent Latin grammarians recognised terminations such as the *o* of *lego*, the *is* of *legis*. So we may wonder why they did not isolate *leg-* by slicing off those endings. One obstacle may have been that the phonology of classical Latin does not generate words that end in this consonant or certain other consonants or consonant groups.⁹ The Sanskrit grammarians transcended some phonological constraints as great or greater in order to pick out a root such as {ric}. Indeed a root is most difficult to abstract, and at the same time most useful, when not just an ending has to be sliced off as in वहसि {vāh|asi} or ueh|is 'you carry,' वहति {vāh|ati} or ueh|it 'he carries,' वहन्ति {vāh|anti} or ueh|unt 'they carry,' etc. To arrive at {ric} in Sanskrit or liqu- in

Latin, you have to allow for different internal vocalizations and for the infixing of a nasal consonant.

No definite link has been shown between the Sanskrit {dhātu} and the term {ʿaṣl} in Arabic grammar. In their technical, philosophical sense -- 'element, principle' -- they were pretty close; but this does not argue any intellectual dependence. For the Arabs did not study Hindu philosophy as they studied Aristotle and Plato, if only in translation -- without the Greek language; still less were the Arabic schools of grammar in a position to take up Sanskrit or to learn from the subtle analysis of Pāṇini and his successors. Arabs, to be sure, invaded Sind as early as A.D. 711/2; but that conquest was abortive. The lasting Muslim conquests in India, by Iranians from the eleventh century on, came too late to modify Arabic grammatical theories. The Arabs even then could not readily have gained knowledge of the language of the Brahmins, had they become curious about it or conceived of the possibility that Sanskrit grammar might throw light upon the structure of Arabic.¹⁰

It appears that the early Arabic grammarians figured out for themselves what constitutes the {ʿaṣl} of a word. Sībawayhi's classic book, written toward the end of the eighth century (around A.H. 170), continually talks about the "root"¹¹ and distinguishes the radical letters from everything else. The emphasis upon letters invites us to put ourselves in the place of those Arabs, who usually read texts without vowel-signs, the Qurʾān being a notable exception. **ذَكَرَ** was either [ḏakara] 'he (has) remembered' or

[ḏukira] 'he has been remembered (mentioned),' besides [ḏakkara] 'he (has) reminded,' [ḏakkir] 'remind' (imperative), [ḏukkira] 'he has been reminded,' while **ذَكَرْنَ** was [ḏakarna] 'they (fem.) have remembered' besides the other vocalizations. Everything but the three consonantal letters **ذَكَرَ** was liable to come and go; the three letters were permanent, like the {ʿaṣl}, the root or trunk of a tree.

Since the grammar of Hebrew was first worked out by Arabic Jews, writing in Arabic, and was modeled upon Arabic grammar, the term {ʿaṣl} was naturally applied to the corresponding phenomenon in Hebrew. The main difference was that in Hebrew grammar the doctrine of triliteral roots, embracing those which appear to be biliteral or even uniliteral, did not find expression until the time of Hayyūj (around A.D. 1000) and was not fully accepted by his immediate successors.¹² The Hebrew language, indeed, exhibits only two letters in many verbs such as **קָם** [qam] 'he (has) stood up,' whereas in the Arabic type that is most nearly cognate a letter *alif* without a consonantal sound comes in between: **قَامَ** [qām].

In the course of time, grammars written in Hebrew were needed by the Jews resident in Christendom, who knew little or no Arabic. {šōreš} in this technical sense served to translate {ʿaṣl}, as in the Arabic title of Ibn Janāh's book, **کِتَابُ الْأَصُولِ**, rendered **סֵפֶר הַשְּׂרָשִׁים** in the version of Rabbi Judah ben Tibbon. The Arabic influence remained dominant; among other things, most Hebrew gram-

marians continued to treat the perfect, not the imperative, as basic. Although זכר can just as well be the imperative [zəḵór] as the perfect [zəḵár, zəḵór] 'he (has) remembered,' and there are some morphological as well as semantic reasons for giving primacy to the imperative, nonetheless the grammars and dictionaries down to the twentieth century give the root as זכר.

If viewed from within Hebrew, whatever vowel turns up in the root of an imperative form seems to have no function but to make the three consonants pronounceable, whereas every perfect form must have an open vowel {o} or {a} either between the first and second radical consonants or between the second and third, if not in both positions:

זָכֶרָה [zoḵeró] 'remember' (coaxing)	זָכְרָה [zəḵeró] 'she has remembered'
זָכְרָנִי [zoḵeréní] 'remember me'	זָכְרָנוּ [zəḵerónu] 'he has remembered us'
זָכְרוּ [ziḵerú] 'remember' (pl.)	זָכְרוּ [zəḵerú] 'they have remembered'
זָכְרוּ [zəḵóru] 'remember' (pl. pausal)	זָכַרְתָּ [zəḵárt] 'you (f. sing.) have remembered'
	זָכַרְתָּ [zəḵórt] " (pausal)
	זָכַרְתָּם [zəḵartóm] 'you (m. sing.) have remembered them' ¹³

The imperfect tense, which is derived from the imperative by pre-fixation of {t-} for 'you' or 'she,' {y-} for 'he,' {ʔ-} for 'I,' and {n-} for 'we,' has many forms with no vowel in the root -- just the minimum transition from one consonant to the next -- provided that there is an accentable suffix; e.g.,

תִּזְכְּרִי [tizḵerí] 'you (fem. sing.) will remember' ¹⁴
תִּזְכְּרֶנּוּ [tizḵerénnu] 'you (masc. sing.) will remember him' or 'she will remember him'
יִזְכְּרוּ [yizḵerú] 'they (masc.) will remember'
אֶזְכֶּרָה [ʔezḵeró] 'let me remember' (coaxing)

So it is tenable to argue that a Hebrew root such as the one that means 'remember' is truly consonantal, but that when it moves into the perfect, a vowel of definitely open quality becomes essential.¹⁵ A little later I will take up Sanskrit roots with no internal vowel.

In Arabic the imperative of any "strong" verb in the "first" form -- besides having one constant vowel between the second and third radical consonants -- has the letter *alif* prefixed. أَذْكُرْ

[ʔuðkur] 'remember,' with the glottal stop and the vowel [u] pronounced, can occur only in an initial position; otherwise أَذْكُرْ

[ðkur], the letter *alif* being still written but cancelled by a superscript mark called {waslah} (e.g., Qurʔān 5.110). So the

Arabic grammarians saw the four letters of the imperative **اذكر** in contrast to the three of the perfect **ذكر** and it made sense for them to identify the latter as the root. Whenever a grammar book was read aloud or a grammatical topic talked about, the root had to take on some pronounceable form, and [ðakara] was preferred to [ʔuðkur].

In Hebrew grammar it was the Arabic model rather than any feature of the Hebrew language itself that gave precedence to the perform form [zəḵār] over the imperative [zəḵór]. Only in roots with a "hollow" -- i.e., biconsonantal -- perfect, such as **קום** [qom], did the grammarians identify the root with the triliteral imperative **קום** [qum].¹⁶

The root, as an abstraction rather than an actual word of the language, would not have to be pronounceable separately. But this theoretical consideration occurs more readily to a foreign linguist, studying the language on paper. The native linguists could not altogether disregard the practical need to pronounce something or other. In Sanskrit, although they went so far as to strip away any suffix, prefix, or even an infixed nasal consonant, still the {dhātū} was a syllable; they never cited it as just two consonants with no intervening vowel. Hence, for example, **पत** {pat} 'fly,' not **प्** {p}, and **तन्** {tan} 'stretch,' not **त्न** {tn}, although they were well aware of actual forms such as **पपतन्** {páptan} 'they fly' (reduplicated aorist; RV. 2.31.1c), **अतन्त** {atnata} 'they (were) stretched' (5.48.2a), in which only the bare consonants of the root are there, plus a syllabic prefix and a syllabic suffix.

The substance or at any rate the conventions of Indo-European linguistics have been affected by the canonical syllabic roots of Sanskrit grammar. By comparison with Greek especially, the Indo-Europeanists have arrived at a root that they cite as *pet- (cf. πέτομαι 'I fly'), or *ten- (cf. τενῶ 'I will stretch'). Neither this vowel nor any modification of it is a required part of all the words in any Indo-European language that share these consonants and this meaning, nor in the reconstructed proto-language. It would be better to cite the root as *pt-, *tn-.¹⁷

When analyzed, the verb-morphology of the ancient Indo-European languages reveals many roots as purely consonantal as the Semitic. Some of the actual words that contain such roots have no vowel between the consonants; of those which have a vowel there, it is one vowel in some, a different vowel in others; e.g., δέομαι 'I am looking,' δέδορκα 'I (have) look(ed).' To be sure, in Indo-European verb-roots the patterns of three mutually compatible consonants are much more restricted: if the first is plosive, the second must be sonant (*d* and *r* in this example); Semitic has no such constraint.

In my view, the best abstraction is not always the simplest but the one that enables us to grasp most exactly how the actual phenomena are related to one another. A linguistic abstraction without vowels, such as {drk}, is valuable insofar as it shows

what δέχομαι and δέδορα have in common with each other and with the aorist tense ἔδρακον 'I looked.' In this paper I would draw attention to the ease of perceiving a consonantal root in the Arabic and Hebrew script. It took no great genius for the medieval grammarians to arrive at the root ذكّر {ðkr} 'remember' in Arabic, זכר {zkr} in Hebrew; for they could see that sequence of letters undisturbed, no matter whether the word was vocalized one way to mean 'he has remembered' or another way to mean 'he has been remembered' (in Arabic) or the imperative 'remember' (in Hebrew).

The consonantal alphabet for writing the Semitic languages, except Akkadian and the lately discovered Eblaite, has confronted the Occidental linguists with a perennial problem. We measure that alphabet against the European norm, which makes vowels and consonants appear equally important. Some linguists focus upon the characteristic structure of the Semitic languages, which is indeed more amenable than most types of language-structure to consonantal writing. Early in the long history of literacy, the Phoenician, Hebrew, or Canaanite alphabet was not only developed but virtually stabilized in so practical a form as to remain in permanent use.

Its Egyptian and cuneiform predecessors became extinct after serving great civilizations for thousands of years. All the influence of Mesopotamia, with its syllabary of vowels, consonant-vowel and vowel-consonant combinations, did not avail to give the scribes of the western Semitic languages the idea that vowel-sounds should be indicated. Not even in Aramaic, though so closely associated with its elder sister Akkadian, was the alphabet basically reformed to take account of vowels; only the weakening of the sounds [ʔ, h, w, y] in certain positions left the letters א, ה, ו and י available for quasi-vocalic functions as well. But an adequate vowel-notation -- still optional in the main -- was devised only through Greek influence upon the Syriac Christians, and from there upon the Arabs and the Jews.¹⁸

The Phoenician or Hebrew alphabet, in Aramaic (particularly Nabataean) lettering, had already been adapted to Arabic earlier in the Christian era, when the Arabs were still mostly pagan.¹⁹ They kept the consonantal principle unaltered; and this Arabic alphabet, in company with Islam, has prospered far and wide. The Semitic peoples were never in any practical position to go over to a vowel-and-consonant alphabet.²⁰

The consonantal alphabet, in origin at least, had one outright advantage: it communicated the Semitic root-morphemes more sharply to the eye than if the vowels were shown in between, as we do in our transliterations ḏakara, ʔuḏkur, etc. The reader, as well as the scribe, could associate the meaning with the sequence of three (or two) letters. The more prominent the notation of the vowels, the better they bring out the difference between the related forms -- in this case, the perfect and the imperative. But if the meaning 'remember,' which they share, is paramount, the consonants that carry it deserve to be written together. The sub-

sidiary morphemes, consisting of vowels, are not, to be sure, better left unwritten, but rather written in some subordinate manner.

Such a vowel-notation is, of course, what was indeed fashioned supplementarily for each of the Semitic languages that lasted into the middle ages. All those systems of vocalization are open to Occidental criticism for the tiny size of the vowel-signs; but otherwise they are well fitted to bring out the relation of the subsidiary morphemes to the root-morphemes -- more clearly in fact than our transliterations do. We could resort to capital letters -- e.g., {ḌaKaRa}; but seldom does it occur to us as worthwhile. Occasionally, in a grammar, you will see such a device for differentiating the root, but not in a transliteration of an actual text, even a brief one. So much do we take for granted the equality of vowels and consonants.

In many languages a vowel is as firm a part of any root as a consonant. We have hundreds or thousands of such roots in English: *beg*, *fold*, *cut*, *pinch*, etc. But that was not so in the older stratum of the Indo-European languages, from which English too has some impressive survivals of Ablaut; e.g., *bear* ~ *born* ~ *burden*. No one would favor writing *b̄rn* or the like, after the Semitic model (specifically, the Jacobite vocalization of Syriac). The European languages have on the whole evolved more and more toward a morphological structure that treats the vowel-sounds on a par with the consonants. Those who adapted the Phoenician alphabet to Greek, with five or seven vowel-letters, were already on the right track, in that the pervasive Ablaut of E/O, H/Ω, etc., was beginning to wane; e.g., no more perfects like λέλοιπα or δέδορκα were formed after the Homeric age, and the perfect tense altogether -- including any inherited forms with Ablaut -- became rarer and rarer after the fourth century B.C. (except for the classicizers known as Atticists in the Roman period).

But granted that a vowel-and-consonant alphabet is generally good for the languages of Europe (among many others), a science of linguistics which lays claim to universality ought to face a crucial question about the morphological function of consonants and of vowels: Besides certain languages with consonantal roots and more languages with roots consisting of vowels and consonants, why are there none in which the roots are vocalic and the consonants serve only for subsidiary morphemes? Not even the languages with the fewest consonant phonemes, such as Hawaiian with only /p k ʔ h l m n w/, form their roots or bases out of vowels exclusively.²¹

Through practical familiarity with the Hebrew and Arabic scripts and through appreciation of their phonological and morphological finesse,²² the linguists of the Occident will best understand the nature of roots, not only in Semitic but in Indo-European and elsewhere. Similarly their sense of the syllable as a unit will gain enormously if they learn to read Sanskrit in Nāgarī letters, or other languages of India in related syllabaries.

NOTES

¹The earliest documentation of שׁוּרָשׁ in a Latin text (incorrectly

pointed) is in Conrad Pellicanus' little known booklet, *De modo legendi et intelligendi Hebraeum* (Strassburg, 1504; repr. Tübingen, 1877, with a preface by Eberhard Nestle), p. [4]; he renders it by the adjective *radicales* (sc. *litterae*).

Sebastian Munster, whose many annotated translations from the Hebrew of Elias Levita appeared from 1520 on, was most instrumental in getting the idea of the root across. E.g., in *Grammatica Eliae Levitae quam ipse appellat Librum electum* (Basel, 1543), oratio prima, Munster's scholium on Elias 1 explains: "Radicem uocant Hebraei primariam aliquam & nudam ipsam dictionem, à qua per adiectionem literarum fluunt personae, numeri, genera, nomina, & reliqua id genus discrimina." *Opus grammaticum consummatum ex uariis Elianis libris concinnatum* (Basel [1549]), p. 40: "Radicem Hebraei uocant שׁוֹרֵשׁ quae scilicet tres habet fundamentales literas in suo themate."

Munster's famous predecessor, Joannes Reuchlin, *De rudimentis Hebraicis* (Pforzheim, 1506), used the terms *primitium* (pp. 571, 586, 599, etc.) and *literae substantiales* (pp. 597, 601, etc.) but not *radix* or *radicales*. On the whole he understood the structure of Hebrew nearly as well as Munster, Clenardus, etc., although he was wrong on quite a few details. For a convenient list of Hebrew grammars down to the year 1556, see Louis Kukenheim, *Contributions à l'histoire de la grammaire grecque, latine et hébraïque à l'époque de la Renaissance* (Leiden, 1951), pp. 139-141.

²*Trunk* would have been, it seems to me, an apter term than *root*, if you think of the many actual verb-forms as twigs growing out of it. But the Hebrew word {šōrēš} did not take in more of the tree than just the root; and the same holds for *radix*, the only Latin word available to translate שׁוֹרֵשׁ.

³Preface to *A Code of Gentoo* [= Hindu] *Laws* (London, 1775), p. xxiii, "The Shanscrit language . . . far exceeds the Greek and the Arabick in the Regularity of its Etymology, and like them has a prodigious Number of Derivatives from each primary Root." Also he speaks of Sanskrit roots often in *A Grammar of the Bengal Language* (Hoogly, 1778; repr. Menston, 1969), pp. vi, 52, 101, 126, 130-138 (a list of more than a hundred roots, along with infinitives and Bengali derivatives). He had learned some Arabic before he went to India.

⁴This is not to disparage the use of the Cyrillic alphabet by some Russian linguists for the phonology of various languages. But they scarcely expect non-Russians to follow it; and they themselves are at home in the International Phonetic Alphabet -- i.e., the modified Latin.

⁵As between the Latin and its predecessor, the Greek alphabet, the wider spread of the Latin was due to political circumstances rather than to any intrinsic superiority; for they differ only in detail, not in principle.

⁶The Dhātupāṭha, or classified catalogue of roots (the first is √ {bhū} 'be'), doubtless existed before Pāṇini, though not neces-

sarily as complete as it was later.

⁷*De lingua Latina* 6.5(37): "Primigenia dicuntur uerba ut lego, scribo, sto, sedeo et cetera, quae non sunt ab ali<o> quo uerbo sed suas habent radices. Contra uerba declinata sunt quae ab ali<o> quo oriuntur, ut ab lego legis, legit, legam et sic indidem hinc permulta." Besides 'gather' the verb also has the less antique meaning 'read,' which Varro is undoubtedly thinking of here.

⁸No ancient Greek grammarian is known to have used ῥίζα 'root' as a technical term. But the polymath Varro scarcely needed any such precedent; for he was an avid agriculturist and fond of likening words to plants (5.1[13], 10[74], etc.).

There have been reflections, now and then, of Varro's use of *radix*. Thus Jehan Palsgrave, *Lesclaircissement de la langue francoyse* ([London] 1530, written in English for Englishmen, in spite of the French title; repr. in *English Linguistics 1500-1800*, no. 190, Menston, 1969), introd. p. [11; according to *OED*, p. 31], means by "rotes" what we might rather call principal parts: "The seconde thorowe all his coniugations is hoolly ruled by y/ and hath his chefe rotes also euer of many syllables and equal/ as Ie cōuertys/ iay conuert/ conuertye."

⁹In pre-classical Latin the meter of Plautus treats words that end in -ē, such as the imperative *lege*, as if that final vowel could optionally be dropped in most environments.

¹⁰Their adoption of the Hindu numbers, like the later adoption of the same numbers 1 2 3 4 5 6 7 8 9 0 by Europeans as the "Arabic" numbers, was such an eminently practical change as to need no great intellectual effort.

¹¹*ʾaṣl* 564 times, according to Gérard Troupeau, *Lexique-index du Kitāb de Sibawayhi* (Études arabes et islamiques, série 3, Études et documents, VII; Paris, 1976), p. 32.

¹²Joseph & Hartwig Derenbourg (edd.), *Opusculs et traités d'Abou 'l-Walīd Merwān ibn Djanāḥ (Rabbi Jônāḥ) de Córdoba* (Paris, 1880), pp. x-xii, xxii-xxv. Kuckenheim (above, note 1), pp. 105-106, 113.

¹³The perfect tense of this Hebrew verb, like *meminī* in Latin, often corresponds in meaning to the English present 'remember(s).'

¹⁴The accent need not actually appear on the suffix. It can be postponed to the next word, as in: לֹא תִזְכְּרֵנִי עוֹד [lō ʔizkeri-ʔod] 'you will remember no longer' (Is. 54:4, Ezek. 23:27; cf. Jer. 3:16).

¹⁵A verb of the stative class may have {e} instead of {o} between the 2d and 3d consonants: שָׁכַחַת [šōkexot] 'she has forgotten' (Pr. 2:17), שָׁכַחַנִי [šēkexoni] 'he has forgotten me' (Is. 49:14).

¹⁶Here the Arabic imperative is biliteral, قُمْ [qum], somewhat like the rare spelling قُمْ in the Bible (Joshua 7:10, 13).

¹⁷Forms like तारोऽ 'stretched' or 'stretchable' (तारः {tatāh} in Sanskrit), τέταται 'it is (has been) stretched,' with no [n];

are still derivable quite simply from **tn̥*-. It is roundabout if you make **ten*- the starting point, and cumbersome if you give equal place to the alternants **ten*-, **ton*-, **tn̥*-. The root is best conceived as that which they have in common: **tn*-. The Indian grammarians, though ignorant of the cognate languages of Europe and having no syllabic [ṇ] in their own, deserve credit for deriving the lesser त- {ta-} or {tatāḥ} from the greater तन् {tan}, by reduction, rather than the other and more usual way of building up the greater from the lesser. They had the syllabic [ṛ] in their language and easily recognised roots such as तृ {tr̥} 'cross,' कृ {kr̥} 'make,' rather than तर, कर {tar, kar}.

¹⁸In Ethiopic, however, the Greek influence (partly through Coptic) resulted in obligatory vowel-signs attached to the letters, not superscript or subscript as in Syriac, Arabic, and Hebrew.

¹⁹Nabia Abbott, *The Rise of the North Arabic Script* (University of Chicago, Oriental Institute Publications, vol. 50 [1939]), pp. 4-13.

²⁰An isolated exception is the Arabic dialect community of Malta, which is all Christian. One by-product of the Zionist revival of Hebrew is a series of schemes by individuals to remedy the defectiveness of the twenty-two letter alphabet; none of these proposals has ever caught on. See Werner Weinberg's survey, "A Bibliography of Proposals to Reform the Hebrew Script," *Studies in Bibliography and Booklore*, 10 (1971/2), 3-18.

²¹Samuel H. Elbert and Mary Kawena Pukui, *Hawaiian Grammar* (Honolulu, 1979), pp. 10, 35.

²²The Syriac script, especially with the Nestorian pointing, deserves equal respect in its own right and is prior in time to any Arabic or Hebrew vowel-notation; see J. B. Segal, *The Diacritical Point and the Accents in Syriac* (London Oriental Series, 2; 1953). Unfortunately the texts, being mainly translations from the Bible and the Greek church fathers, will scarcely attract many readers as original works of genius do.

CONTRIBUTION TO THE HISTORY OF THE STUDY
OF THE ADJECTIVE IN JAPANESE

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1. Introduction

The purpose of this paper is to examine one of the contributions to general Linguistics and, more specifically, to Japanese Linguistics evidenced in João Rodriguez' Arte da Lingoa de Iapam (Nagasaki 1604-1608). My particular concerns are Rodriguez' treatment of the notion ADJECTIVE and the manner in which his analysis reflects intelligent procedural departures from the traditional paradigm. In his recent brief comparative evaluation of Rodriguez with another 17th century Jesuit missionary student of the Japanese Language, Diego Collado (Ars Grammaticae Iaponae Linguae, Rome 1632), Spear (1975:12-13) notes the "failure" of both scholars in finding a satisfactory description of the ADJECTIVE and the PREPOSITION in Japanese. This putative short-coming Spear attributes to the application of the traditional Latin grammatical model to Japanese which is certain to be found wanting because of the structural differences between Latin and Japanese, differences which the Latin paradigm was not designed to handle.¹

"With respect to the adjectives, Collado attempted to deal with their functions in the manner appropriate to Latin, that is as a sub-class of nouns. He also recognized their formal similarity to the verb and treated them briefly as a sub-class of the substantive verb, but his heavy reliance on the semantic categories of Latin did not permit him to follow Rodriguez who was able to recognize more clearly their formal as well as their functional distinctiveness."

Rodriguez analyzed Japanese adjectives as a sub-class of verbs. It is this analysis of some Japanese adjectives as a sub-class of verbs that I wish to concentrate attention on here. These adjectives, in particular those ending in NA, even today constitute one of the most controversial aspects of the Parts of Speech in modern Japanese grammatical studies. I will demonstrate that Rodriguez not only provided the original taxonomy/nomenclature but also that his analysis is prototypic from the point of view of Linguistic Typology, at least in the area of European-Japanese Typology, and is a significant contribution to linguistic procedure, anticipating by three and a half centuries analyses proposed in present-day Transformational Grammar.

2. The most penetrating of the earlier commentaries on Rodriguez' theory of the ADJECTIVE is Doi Tadao (1942). Doi observes that, while Rodriguez' description of the Japanese adjectives follows in the main the Latin Grammatical model, in the description of what he called the ADJECTIVAL VERB he attempted a most interesting and innovative analysis. Indeed, the common criticism that his work merely represents an adaptation to Japanese of Latin Grammar (as laid out in treatises such as Alvarez' De Institutione Grammatica (Venice 1575) misses the point and fails to afford Rodriguez the credit he deserves for his deviation from tradition in the specific case of Japanese ADJECTIVES and for the full linguistic typological/universal and analytical implication of this divergence. The notion of ADJECTIVAL VERB is patently and overtly central to Rodriguez' "theory" of the ADJECTIVE. It was he who first established the opposition between NOMEN ADJECTIVUM (the traditional Latin categorization of ADJECTIVE) and VERBUM ADJECTIVUM and the distinction arises out of his comparative Japanese-to-Romance (Latin, Portuguese) study. Thus, a structural peculiarity of Japanese (vis à vis Lat./Port.) led Rodriguez to a new grammatical sub-class and also to further analysis of ADJECTIVE in both Japanese and Portuguese which involves the notions of Deep Grammar and Transformational Derivation.² It must be emphasized that Rodriguez does not set up the VERBUM ADJECTIVUM as a new Part of Speech (PARS ORATIONIS) but as a sub-division of the PARS ORATIONIS : VERBUM. Neither the term ADJECTIVE nor the composite term VERBUM ADJECTIVUM occurs in his list of the ten Parts of Speech of Japanese.³ As I will demonstrate below, this is consistent with Rodriguez' clearly implied transformationally-derived status for the set of adjectives subsumed under the division VERBUM/ADJECTIVUM as opposed to the DEEP, non-transformationally-derived status for the NOMEN ADJECTIVUM set of "adjectives".

3. Specifics of Rodriguez' Analysis

3(i) Rodriguez classifies Japanese adjectives into two major categories:

- a) The adjective proper - taking the particle NO. This category is indeclinable. It is possible to claim that it corresponds to the Latin (NOMEN) ADJECTIVUM class despite the fact that the Latin adjectives are inflected for Number and Gender within the Phrase.
 Examples: Moromoro-NO fito "various people"
Makoto-NO deus "true God/God of Truth"
 (Portuguese translation equivalent Deus verdadeiro or Deus de verdade)

These Rodriguez considers to be "true" Japanese adjectives-basic and non-derived and not members of the sub-class VERBUM ADJECTIVUM. He also treated as belonging to this category a limited set of lexical compounds such as vobune "big ship", kogatana "small knife", voame "big rain" etc.

- b) The category VERBO ADJECTIVO or, alternatively, VERBO SUBSTANTIVO ADJECTIVADO for which Doi gives the Japanese translation equivalent KEIYŌSONZAIDŌSHI. This corresponds to the set of adjectival functions manifesting its own conjugation as to tenses and moods. In this analysis it is clear that Rodriguez is not relying solely on vague semantic criteria nor even on semantic criteria alone. Rather, he takes into account the observed correlations in Japanese between formal morphophonological/morphosyntactic and semantic properties. The members of the category VERBUM ADJECTIVUM is inflected like the members of the class VERBUM and they are inflected in both the predicate and the attributive positions. This principle of "analytical analogy" is of course at the base of the formal association of the adjectives with the NOMEN class in Latin- the former being inflected for number and gender as are the nouns but not for tense, mood, etc. as are the members of the class VERBUM. Rodriguez' forte as a linguist comes to the fore in his willingness to accept the logic of the analytical analogy and conclude that when some members of a general category, in the present case "adjective", look like nouns formally (e.g., indeclinability as in a) above) they must be classed as or with nouns, but when others look like verbs formally (e.g., inflection for tense, mood, etc.) they must be classed as or with VERBUM. It is the FORM-TO-MEANING correlation that determines the correct classification and not the apriori assumption of the universality of Latin Grammar. It is possible therefore to characterize Rodriguez' contribution as realizing the "correct" descriptive form of traditional Latin Grammar, eschewing presumptions of universality and appropriately making changes in the form of the descriptive taxonomic framework as they are required by the observed characteristics of the language, i.e., in this instance Japanese.

Rodriguez divided the ADJECTIVAL VERBS further:

- b.1 Those ending in -AI -EI -IJ -OI -UI
- 2. Those ending in NA or NARU

While it is clear that this division is perfectly justified on etymological/diachronic -linguistic grounds⁴, it is just as clear that Rodriguez' applied criteria are again purely formal morpho-(phono)logical with clear form-meaning correlation in the case of b.2 sub-class.⁵ The application of the analytical process leaves a set where the form-meaning correlation is not as clear (if at all). However, they still constitute a formal sub-class and this for very clear formal reasons and it is to Rodriguez' credit that he does not shy away from the logical implications of his descriptivist approach. Thus, for the b.2 NA/NARU set, Rodriguez' classification as VERBO SUBSTANTIVO ADJECTIVADO appropriately reflects the synchronic and diachronic linguistic facts. The "particle" NA/NARU is derived from (parts) of the verb of existence NARU(NARI). It also corresponds generally to the Portuguese copula-verbs SER and ESTAR, and also to Latin ESSE which is called the verbum substantivum in Alvarez' Latin Grammar. Furthermore, the term KEIYŌSONZAI DŌSHI (adjectival existential verb) which Rodriguez created for these adjectives is a most appropriate descriptive one.

3(ii) Details of Rodriguez' Morphology of VERBUM ADJECTIVUM forms. In the paradigmatic representation of Japanese adjectival verbs, Rodriguez closely follows the Latin system of conjugation. At the outset, he states that the only forms that are proper to the conjugation of the adjectival verbs in NA/NARU are: the present indicative, e.g. aqiraca-NA, the conjunctive, e.g. aqiraca-NAREBA, and the gerund or participle, e.g. acquiraca-DE. All other forms are conjugated with the help of appropriate particles and verbs of existence. All tenses and moods can be made "elegant" by the use of the verb of existence, following the ending NI, or the root.⁶ Rodriguez interprets aqiracani as the root of the VERB, noting that this form functions also as an adverb: aqiracani yu "speak clearly". Evidence from derivational morphology and from syntactic phenomena, however, seems to favor aqiraca as the root:

aqiraca+na	"...is clear"
aqiraca+sōna	"seemingly clear; appear to be clear"
aqiraca+nagara	"although it is clear..."
aqiraca+raxij	"looks clear"
aqiraca+de	(gerund, participial)
aqiraca+sa	(nominalization) "clarity" ⁷

The verb NARU is central to the description of the adjectival verbs, but unfortunately, Rodriguez is not entirely clear on this point. He notes that the particle (joshi) NA/NARU is of the verb of existence, NARU, which is represented by a special Chinese character. While Rodriguez does not specify the character, Doi suggests that it may be yēh (也), a kind of copular verb in Classical Chinese and one which is also the copula at the end of the sentence in Japanese as well as Chinese. Rodriguez further observes that NARI occurs in sentence final position in the

written language, but in the spoken language, the simplified form NA (from NARU) is the usual form, e.g. Mizu kireina "The water is clear", Dōri aquiracana "The reason is clear", etc. In the listing of existential verbs that were available in the spoken language of the period, we find naru, nari, aru, gozaru and the negative counterparts, nai and gozanai, which are used in conjunction with adjectival verbs.

3(iii) The syntax of the VERBUM ADJECTIVUM. We noted above that the NA/NARU sub-class involves a copular or substantive verbal element. It appears to be then a very short step to the conclusion that these adjectives, and, because of their functional syntactic similarity, the members of the b.1 (see above) sub-class are derived from subordinate predicates. In Rodriguez, we find the following "analyses".

a) If the sentence has only fucai, it means in Portuguese está (or he) fundo, ("It is deep"), the subject being understood or implied. Fucai is the predicative adjectival verb. "The river is deep" is rendered in Japanese as Fucai kawade gozaru, which corresponds to the Portuguese He rio fundo. Fucai in the Japanese sentence is to be considered a relative clause, which, in Portuguese, is: He rio [que está fundo] "It is a river [which is deep]". (Notice that, in English, It is a deep river may be derived from the underlying relative clause, river [that is deep] by the Relative Reduction Transformation and the Modifier Preposing Rule.) The adjectival verb fucai in both the predicate and attributive, i.e., prenominal positions, have an identical form.

b) Similarly, the adjectival verbs ending in NA/NARU: Aquiracana or aqiracanaru dōride gozaru, "It is clear reason", corresponds in Portuguese to He razão clara. The Japanese sentence has, as it were, two uses of the existential verb: one in the adjectival verb aqiracana or aqiracanaru and gozaru, which is an honorific form of the verb of existence ari, aru. Aquiracana is likewise regarded as being derived from the relative clause It is a reason [that is clear], which, in Portuguese, is He razão [que he clara]. Rodriguez notes that this characteristic phenomenon of Japanese syntax can be made clear by contrasting it with the corresponding use in Portuguese.

c) The basic resemblance of the adjectival verbs, discussed above, and other verbs, i.e., verbs of action, can be seen in the use of the verbal form in prenominal position. This too is regarded as a relative clause, although the same form is elsewhere listed as a present participial form.

Yomu fito means the same thing as O homen que le, that is "The man who reads". In the predicate use we have Fito-ga yomu "(the) man reads". All other tenses follow the same rule, i.e., the predicate forms and the attributive forms are identical.

From this, we observe that Rodriguez had a clear notion of Deep Grammar relations and that his grammar was not a purely static, descriptive one. It must be noted, however, that there is a clear connection between the first decision to distinguish between the NOMEN ADJECTIVUM and the VERBUM ADJECTIVUM and the establishment of a subordinate sentential source for the members of the latter class. Thus, Rodriguez' interpretation of the pre-nominal verbs, including the adjectival verbs, as being derived from the relative clauses, is original, extremely insightful for his time and clearly anticipates the modern day Transformational approach.

Rodriguez arrived at his novel description and analysis of the adjectival verbs by contrasting Japanese and Portuguese (his native language) and, to a lesser degree, with Latin. This comparative approach is in sharp contrast with the general (e.g. Collado) description based on the traditional Latin grammatical model. What we have then in Rodriguez' work is an outstanding early example of the value for linguistics of the inter-language/typological approach. Not only does it provide us with new insights into the nature of Language but also figures as an excellent paradigm of the poverty of assuming-within a highly qualitative, descriptive approach - the universal applicability of a grammar that is based solely on the consideration of one language.

NOTES

1. As to the PREPOSITION, I merely point out here that Rodriguez was, as far as I have been able to determine, the first linguist to use the term *posposição* (POSTPOSITION) to denote those particles which, in Japanese, function in a manner similar to the Latin Preposition. According to the OED, the term POSTPOSITION in the grammatical sense does not appear in English before 1846.
2. Collado, on the other hand did not choose to employ the term adjectival verb while recognizing that at least in the predicate position the Japanese adjective is conjugated like verbs.
3. Note also that they do not occur in Collado's list.
4. The diachrony of b.2 is very clear and is still obviously a form of the copula in the modern forms of the language. The etymology of b.1 is not as apparent.
5. It should be noted here that Collado also appears to be aware of the "verbality" involved in this set: "Si uero uerbum huiusmodi non sequatur, eundem facit sensum, quia adiectivum includit in se uerbum substantivum". Spear (1975:115-6) translated the passage as "But if this kind of verb does not

follow, the sense is not altered since the adjective is used as a substantive verb". The Latin original "includit in se uerbum substantivum" should probably be more adequately rendered by "includes in itself (a) substantive verb". The notion that the predicative adjective, in Japanese, contains the idea of the copular verb, to be, is a very common one for speakers of most European languages, in which the copulas (s) are required in the predicate use of adjectives. In this respect, Collado's semantically oriented interpretation of the Japanese adjective is quite understandable.

6. The only significant differences between the paradigm of the colloquial speech and that of the written, classical language is that the latter has aquiracanari in the present indicative and aquiracanari keru in the preterite, whereas in the spoken language, aquiracana and aquiracani atta, are used. The entire conjugational system of the adjectival verb is in fact that of the verb of existence, i.e., naru and aru. Doi (1942:224-5) recapitulated the conjugational system of the adjectival verbs ending in NA/NARU, e.g. aquiracana based on the data in the Arte Grande and the Arte Breve (Macao 1620). The form aquiracani aru, lacking in the Arte Grande, is listed in Doi's table as well as the polite or elegant form: aquiracani gozaru, for the present indicative. The alternative forms for the same tense are aquiracana and aquiracanaru. However, as Rodriguez noted, naru does not occur in sentence final position, but it may be used as a formal variant of NA in prenominal position, e.g., in relative clauses. In modern Japanese, the adjectival verbs follow the Dana conjugation, that is, Na in the attributive, prenominal position, and Da in the sentence final, predicative position: Aquiracana + Noun: Aquiraka-Da.
7. Collado also gives a brief account of the morphology of the adjectives ending in NA. "There are also adjectives ending in na which, when they are placed before nouns, do not alter the construction; e.g. quirei na coto 'a clear thing;... Adjectives which end in na change to ni when they are placed before verbs; e.g. foxin ni zonzuru 'I think it doubtful'. (Spear 1975:115 .) However, he completely ignores the verbal nature of NA, describing it merely as an adjectival suffix (as is commonly done even today in modern Japanese pedagogical grammar).

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FROM VSO TO SVO IN FRENCH: A FUNCTIONAL APPROACH

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It has been commonly held that the shift in basic word order from SOV in Classical Latin to SVO in Modern French resulted from the direct shift in the position of the verb from final to medial position (i.e., SOV>SVO, e.g., Thurneysen 1892; SXV>TVX>SVX, e.g., Vennemann 1973). Explanations offered for this development have attributed causality to external and internal factors such as language contact, the rhythm of the language, or the need to distinguish reliably and uniformly between the subject and object of a construction.

The aim of this paper is twofold. Primarily, I would like to establish that the distributional and functional characteristics of various word order patterns from the Late Latin period to the Middle French period suggest that the VSO order should be posited as a basic word order stage in the evolution from SOV in Classical Latin to SVO in Modern French. It will be shown that if the VSO order is regarded as an evolving basic word order during the Late Latin period (200 A.D. to 500 A.D., 500 A.D. to 800 A.D.)¹ and as the basic word order throughout most of the Old French period (800 A.D. to 1250 A.D.)² accurate predictions can be made about the various word order patterns found during these periods. In addition, it will be shown that in order to account for the data, the SVO order should not be considered the basic word order until the Middle French period (1250 A.D. to 1600 A.D.)³ and furthermore that its generalization to all environments is still proceeding in present-day Modern French, especially in the spoken language. Secondly, I would like to propose an explanation for these developments that, in contrast to other explanations offered to date, is both externally and internally justifiable. The pragmatic explanation that will be given not only conforms to the linguistic data available for these periods but also conforms to what we believe to be true of the structure of linguistic communication.

Lets now proceed to providing evidence for the model outlined above. Extensive studies of the position of the verb in the literary language of Classical Latin times have suggested that the basic word order in standard Classical Latin was SOV. For instance, Serbat (1975) has pointed out that in the works by Caesar (102 or 100 B.C. to 44 B.C.) that he examined, the verb occurred in final position 80% of the time in main clauses and 90% of the time in subordinate clauses. However, this pattern

seems to be changing in popular Classical Latin, where despite the fact that SOV constructions remain very frequent as well as unmarked, the verb seems to be progressively losing its final position. For instance, in Petronius' Cena Trimalchionis, written in the 6th to 7th decade of the 1st century A.D., the verb already occurs in final position only 51% of the time in main clauses and 67% of the time in subordinate clauses and in Terentianus' letters, written in the 1st quarter of the 2nd century A.D., the verb occurs in final position only 15% of the time in main clauses and 33% of the time in subordinate clauses.⁴ What seems to be particularly interesting, however, is the frequent initial position of the verb in the popular Classical Latin works and, furthermore, the environments in which this occurs. For example, Ramsden's (1963) study suggests that, in the Cena Trimalchionis, juxtaposed object pronouns occur postposed to the verb 49% of the time and that in 82% of these cases the verb can be considered the initial constituent.⁵ An examination of verb initial constructions in the Cena Trimalchionis suggests that the verb occurs initially mostly in environments where verb topicalization for pragmatic considerations would tend to occur. Thus, for instance, the verb occurs initially in independent clauses when its focusing is desired, as in

- (1) Petr. CT 150 Haec ut dixit Trimalchio, flere_v coepit
ubertim. Flebat_v et Fortunata, flebat_v
et Habinnas.
'As he finished speaking, he burst into a
flood of tears. Fortunata cried too, so
did Habinnas.'

and in coordinate constructions or following subordinate clauses where the initial verb, which is marked for person and can function anaphorically, serves to link discourse, for example:⁶

- (2) Petr. CT 146 Et commendo_v illam omnibus amicis meis
'And I recommend her to the kindness of
all my friends.'

I have suggested that in popular Classical Latin SOV still seemed to be the basic word order but that verb initial constructions, although mostly marked, were becoming increasingly more frequent.⁷ By the 3rd century A.D., SOV constructions had become too uncommon in works reflecting the Vulgar Latin of the Late Latin period to still suggest a basic SOV order in most environments. For example, as noted by Ramsden (1963), in Itala, written during the 3rd century A.D., the verb occupied the final position only 13% of the time in main clauses and 26% of the time in subordinate clauses. Furthermore, his study suggests that in Itala, where object pronouns were juxtaposed to the verb 90% of the time, they were found postposed

to it 91% of the time. The postposition of object pronouns tended to occur not only in environments where previously it had resulted from verb topicalization but also in environments where verb topicalization could not have played a role, such as following a full direct or indirect object, a predicate complement, or the subject of the verb, elements themselves topicalized. It is important to note at this point that SVOp constructions were fairly uncommon in Itala. For instance, only 7% of the object pronouns postposed to a verb occurred following a subject. If the verb had been moving into medial position, as has been claimed, rather than into initial position as I am claiming, such constructions would have been very common. It appears that the only environment where object anteposition was not only common in Itala but also unmarked was within a relative clause. As has been suggested by Givón (1977) relative clauses are quite conservative in terms of word order change based on topicalization. A further change reflected in Itala which argues for a developing basic VSO order is the frequent and inherently unmarked nature of V(S)O(p) constructions in most environments, especially in those environments where previously such constructions had been marked. Ramsden (1963) notes for instance that in Itala, where following a coordinate conjunction or a subordinate clause the object pronoun occurred postposed to the initial verb in 89.5% of the cases, if it occurred anteposed to it it had a marked function.

Lets review what I have argued so far about the word order patterns found in Vulgar Latin. SOV constructions which were very frequent and unmarked in Classical Latin had become fairly infrequent in Vulgar Latin except in relative clauses, which we know to be conservative in terms of word order change. In contrast, V(S)O constructions, which were frequent but marked for the most part in popular Classical Latin, had become inherently unmarked and very frequent in Vulgar Latin. If we postulated that the basic SOV order of Classical Latin had evolved into a basic SVO order, we could not account for the frequency and lack of markedness of VSO constructions, nor for the relative infrequency and markedness of SVO constructions, which mostly occurred when the subject was functioning as a topic. However, if we posited that the VSO order was the basic word order we could account for the distribution and function of those word order patterns. We could also account for the presence of marked OVS(O) constructions which occurred quite frequently in Vulgar Latin (VSO>OVS(O)) as well as for the absence of OSV(O) constructions which we would expect to occur within a SVO system. We will see later that when the SVO order did become the basic word order, OVS(O) constructions became rare in most environments while OSV(O) constructions became frequent. The development towards a basic VSO order can be readily explained if we consider language to be a communicative system. I have proposed that at first verb initial constructions were the result of verb topicalization for pragmatic considerations and that later these

constructions became generalized and unmarked in most environments resulting in a basic VSO order. This explanation is internally justifiable as verb initial constructions were found at first in just those environments where we would expect a verb marked for person to be topicalized, especially in a discourse oriented system such as Vulgar Latin, and also because the levelling of paradigms was the rule rather than the exception in Vulgar Latin. The pragmatic explanation offered is also externally justifiable as it accords with what we believe to be true about the nature of communication. I would like to note briefly at this time that the models proposed to date not only suggest that the verb moved internally, which we have seen does not account for the data, but also have advanced explanations which are not justifiable. For instance, Vennemann (1973) has proposed that the loss of verb final position in Indo-European languages occurred due to topicalization problems resulting from the reduction of S-O morphology and that to solve the problem the verb moved medially. There are a variety of problems with this hypothesis. For example, as suggested by Comrie (1981), there are VO languages among the Indo-European languages that still maintain the nominative-accusative distinction. Furthermore, Vennemann's hypothesis could not hold for Late Latin, as the verb moved out of final position at a time when little or no weakening of the nominative-accusative distinction had taken place.⁸

Throughout most of the Old French period the basic word order is distinctly VSO. As noted by Kukenheim (1968), early Old French (800-1150 A.D.) favored orders VSO, OVS(O), and SVO, with OVS(O) being highly favored and occurring much more often with nominal rather than with pronominal subjects. Examples of the SOV order are also found, especially during the early Old French period, but mainly in relative clauses where this order was also mostly found in Late Latin. Foulet (1974) suggests that in the spoken language such constructions were basically nonexistent. Constructions with an OSV(O) order are extremely rare and only become more common during the latter period of Old French, as the SVO order becomes the most frequent and the VSO and OVS orders become less common. Zwanenburg (1978) notes, for instance, that in Villehardouin's La Conquête de Constantinople, written at the beginning of the 13th century, there are only 3 examples of this construction in main or independent clauses. These word order patterns clearly suggest a basic VSO order throughout the early Old French period, as such a basic order can account for the frequent and unmarked VSO constructions as well as for the frequent but functionally marked OVS(O) and SVO constructions, which can be described as resulting from object or subject topicalization.⁹ Furthermore, a basic VSO order can account for the SOV constructions as being relics of an old SOV order within a conservative environment, as well as for the mainly absent OSV(O) constructions. As suggested previously, a basic SVO order could not account for the frequency of OVS(O) constructions, for

the extreme infrequency of OSV(O) constructions, nor for the unmarked nature of VSO constructions during the early period of Old French.¹⁰ However, postulating a developing SVO order for the end of the Old French period does account for the generalization of SVO constructions during that period at the expense of other orders, as well as for the increase in use of OSV(O) constructions, which occurred especially when the object was an adverbial expression that served to set the stage for the SVO utterance to follow.

How can we account for the change towards a basic SVO order at the end of the Old French period? We get an indication of the answer to this question if we note that initially SVO constructions were especially found when the subject was a pronoun. If we then examine the function and position of subject pronouns during this period, we note that up to the end of the 12th century the subject pronoun tended to be found mostly when it was marked for focus or used as an objective topic and when found it occurred mainly in initial position, which throughout this period was a stressed position. However, as pointed out by Moignet (1965), from the end of the 12th century we find an especially rapid growth of subject pronouns. This change was accompanied by an increasing loss of markedness and by their progressive generalization into initial position, which had become an unstressed position by the beginning of the 13th century due to the new oxytonic rhythm. Only unmarked, unstressed subject pronouns could have become generalized in this unstressed position.¹¹ Thus, independent subject pronouns were the first to undergo a shift to SVO syntax, due to their marked topicalization function.¹² Their increasing generalization into initial position and the consequent development towards a basic SVO order resulted from their increase in use, from their loss of markedness and from changes in the rhythmic nature of the language whereby the initial position became unstressed.

During the Middle French period, the VSO order as well as the OVS(O) order, popular in Old French, become much less common. However, as noted by Gougenheim (1951), the VSO order could still be found fairly frequently during the 16th century in interrogative constructions, following et or or, in simple declarative constructions with no direct or indirect objects, or following a subordinate clause, environments where we have seen the VSO order was especially favored in Late Latin, e.g.,

- (3) Rabelais Gargantua 23 Se esveilleoit▼ doncques Gargantua,
environ quatre heures du matin.
‘And Gargantua woke up at around 4
o’clock in the morning.’

It is interesting to note that the VSO order was also common in relative clauses. Yet, even in that conservative environment,

SVO constructions were quite frequent. The OVS(O) order was also found frequently, especially when the initial object was a predicative adjective, an adverb or an adverbial phrase, a direct or indirect object, or a prepositional phrase, e.g.,

- (4) M. de Navarre L'Heptaméron 1.3.44 Mais maintenant, croit-
elles fermement
'But now she believes
firmly'

However, the most important word order change during the Middle French period was the fixation of the SVO order as the basic word order. This process was certainly encouraged by the extensive loss of inflectional markers which had occurred by the beginning of this period. Concomitant with the generalization and unmarking of the SVO order was the increase in use of OSV(O) constructions, which we would only expect within a SVO system. From an analysis of 13th and 14th century texts, Zwanenburg (1978) notes that the frequent occurrence of OSV(O) constructions in Middle French texts was the most striking word order difference between that period and the end of the Old French period.

Finally, I would like to point out that in present-day formal French, the generalization of the SVO order to all environments is still incomplete. Thus we still find constructions with an unmarked VSO order or a marked OVS(O) order. For example, interrogative constructions such as Où est-elle? 'Where is she?' are the norm. However in familiar or popular spoken French, there is a tendency to level the word order paradigm completely, thus for instance, est-ce que is used to level interrogative constructions in familiar French as in Où est-ce qu'elle est? and, in popular spoken French, simple intonation tends to be used in that environment, thus we have où elle est? It will be interesting to see whether this levelling will take place in the standard language and furthermore whether the tendency to extensively dislocate elements for pragmatic consideration, present in spoken French, will affect the basic SVO order.¹⁵

FOOTNOTES

¹The spoken Latin of the Late Latin period (Vulgar Latin), especially the Latin of the 3rd through the 5th century, is felt to be the ancestral form of the Romance languages. The texts that are examined in this paper for that period reflect the spoken language, which was increasingly differentiating itself from the Classical literary standard. The period from the 6th through the 8th century, known as the Dark Ages, is identified as

a separate period within the Late Latin period as it marks the progressive development of various Latin dialects into independent languages. Unfortunately there is little literary evidence for that period and, thus, most linguistic developments during that period must be reconstructed.

²By the beginning of the 9th century the language spoken in France was so distinct from both literary and spoken Latin that it was considered a new vernacular (*rustica romana lingua*). The justification for viewing the mid- 13th century as the end of that period is offered in Galambos (1981).

³The justification for delimiting the Middle French period in this manner can be found in Galambos (1981).

⁴It should be noted that typological changes seem to have preceded these word order changes. As pointed out by Friedrich (1975), the typology in the works by Plautus (254-154 B.C.) appears to be VO (e.g., prefixing, postposed relative clauses, NG, NA) although the position of the verb is mostly still final.

⁵An initial verb here refers to a verb which is found initially or paratactically joined to the preceding element, to a verb following a coordinate or subordinate conjunction, to a verb following a subordinate clause and to a verb following a negative adverb. It is considered that in all of these environments the verb is the initial constituent.

⁶See Marouzeau (1938) for a detailed discussion of verb initial constructions in Latin. It should be noted that the environments where verbs occurred initially in popular Classical Latin are similar to those where the verb was topicalized in early Indo-European languages as reviewed by Dressler (1969) and Watkins (1976).

⁷There were some environments where verb initial constructions were unmarked. This occurred with performative verbs such as in *scio ego* 'I know', where if the subject occurred initially it was strongly emphatic. It also occurred with imperative verbs.

⁸Other explanations offered include Thurneysen's (1892) proposal that the verb moved into medial position in the pre-Romance period for rhythmic reasons based on enclisis. As noted by Marouzeau (1938) even if rhythmic reasons had influenced word order, they could have not been based on enclisis which no longer played a role at this time. It has also been suggested by Lehmann (1973) that the change from OV to VO in Latin can be explained by contact with the languages displaced by Indo-European languages. There is as of yet no data to support this hypothesis which, as suggested by Vennemann (1973), has the disadvantage of making this word order change appear accidental. For a detailed critique of Vennemann's and Lehmann's theories consult Comrie (1981) and Galambos (1981).

⁹It is interesting to note that in Old French the VS(O) order was used in interrogative phrases even when the subject was a proper name, e.g., *Vialt Yvain?*

¹⁰OVS(O) constructions have been explained previously (Foulet 1974) as resulting from the obligatory transfer of the

subject to final position whenever an autonomous object occurred initially (i.e., SVO→OVS). However, this explanation does not seem to be pragmatically justifiable. The functional validity of a discourse strategy that would commonly topicalize an object and simultaneously dislocate a subject to final position is questionable.

¹¹As noted by Moignet (1965) the generalization of the subject pronoun in initial position occurred first in subordinate constructions introduced by a subordinate conjunction, and only later in main and independent clauses. In these latter constructions we find examples of (O)VSp constructions, especially at the end of the Old French period, indicating the subject pronoun's ability to be used with an unmarked function but its lack of generalization into initial position. For an extended discussion on this question as well as on rhythmic changes that occurred during the Old French period see Galambos (1981).

¹²See a similar suggestion for Biblical Hebrew by Givón (1977).

¹³For a thorough review of word order patterns in familiar and popular spoken French consult Galambos (1980 and 1981).

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CYNEWULF OR CYNEWULFIAN?

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The problem discussed here is two-fold: whether the four signed poems of Cynewulf are the work of the man indicated by the runic signatures, and whether other unsigned poems of apparently the same time period as the signed poems could also be his work. This analysis forms a part of the ongoing study of Old English using the poetic texts for data. This preliminary statistical study indicates that the results presented are possible and that the following hypotheses are applicable to the problem and worthy of further investigation:

- (1) An author's linguistic habits will be much the same from work to work with only minor, inconsequential differences, and each work will have a linguistic profile very similar, if not exactly the same, to those found in other works by the same poet; and
- (2) Works either can be assigned to a particular author or rejected as his work by the linguistic profiles.

The linguistic profiles were developed from a statistical analysis of several competing/contrastive feature sets (Fries, 1970) and the ratios (proportions) of usage between each set's features (Collins, 1982).

Old English provides an excellent data source for an analysis of this type because it is a transitional language, one that is changing its typological organization from the inherited OV (object/verb) typology to the new VO (verb/object) typology, with features from both typologies co-occurring in the form of sets of competing/contrastive features (Lehmann, 1978a; Hyman, 1974). These feature sets are the choices available to the speaker/poet. The ratios of usage of the set members in a changing language will also be influenced by time with the replacing feature increasing in use and the feature eventually replaced decreasing through time. A smooth progression of change is assumed. The validity of this assumption was presented at the 1981 LACUS Forum (Collins, 1982). Since language change occurs through the replacement of one form by another, during the period of change various sets of competing features occur - for example, in Old English, the use of the demonstrative se, seo, þæt in the noun phrase (a VO typological characteristic) vs. the non-use (an OV characteristic):

- (1a) hu þa aepelingas ellen fremedon (Beowulf 3)
[how the princes performed deeds of valor]

- (1b) sele hlifade heah and horngeap (Beowulf 81)
[[the] hall towered high and wide-gabled];

the use of the possessive pronoun (a VO characteristic vs. the non-use (an OV characteristic):

- (2a) þaet him his winemagas georne hyrdon (Beowulf 65)
[so that his retainers eagerly heard/obeyed him]

- (2b) on faeder bearme (Beowulf 21)
[in [his] father's possession];

the occurrence of object nominal case alternation (an interim condition in a language moving from inflection to word order) vs. the use of a specific case for the object nominal always associated with a particular verb (an OV characteristic):

- (3a1) þa gen sylf cyning geweold his gewitte [dative object]
(Beowulf 2702)
[then yet the king himself controlled his intellect]

- (3a2) swa he ne mihte waepna [genitive object] gewieldan
(Beowulf 1508)
[as he could not control [his] weapons]

- (3a3) ond halig God geweold wigsigor [accusative object]
(Beowulf 1553)
[and holy God controlled the victory]

- (3b1) þaet he beahhordes [genitive object] brucan [verb] moste
(Beowulf 894)
[so that he might enjoy [verb] the treasure [object]]

- (3b2) Bruc [verb] pisses beages [genitive object] (Beowulf 1116)
[Enjoy [verb] this ring/bracelet [object];

(cf. Collins, 1980; Visser, 1970, for multiple examples).

As this study is concerned with the language of Old English poetry and the changes in the language used in the poetry, it is expected that the poetic form places constraints on language usage and that the impact of these constraints has to be evaluated. For example, the placement of the adjective or genitive noun either before or after its head noun and in the same line as the head noun or not is a matter of poetic form: line alliteration and stress (Lehmann, 1956, 64-123). However, the poetic form constraints could not be too restrictive as Old English poetry was an oral poetry and, therefore, had to conform to ordinary speech sufficiently enough so that the primary purpose of lan-

guage, communication, would not be lost.

For a poet of a particular time period, even if the specific dates of his works are unknown, the ratios of usage between members of a competing set should be consistent from poem to poem in a demonstrable fashion. These ratios all together make up the linguistic profile of a particular work; this profile should be repeated in other poems by the same author. No two poets, even though of the same time period, should show exactly the same linguistic profile. Idiosyncratic differences in usage should distinguish between two poets even though overall they may both fit within the same time period and, therefore, have profiles consonant with that time period.

Because much of speech and writing is habitual behavior and because one of the important cultural norms occurring throughout the entire Old English period was that deviation from established custom was to be avoided in all aspects of life, a poet would not indulge in innovative linguistic practice for the sake of variety as is expected and rewarded today. This theme pervades both the prose and poetry throughout the period concomitantly with the idea that deviation would upset the natural order of life, bringing about an unwanted and undesirable chaos in which religious heresy, political upheaval, and catastrophic social change would flourish - a state to be avoided at all costs. Therefore, for Old English the poetic works of one man should present related linguistic profiles.

The signed poems of Cynewulf and other unsigned poems that have been at one time or another placed in the Cynewulf Canon provided the data for testing the hypotheses. The literary history of which poems make up the Canon and which ones belong to the Cynewulfian School is one fraught with instability and controversy. There are four poems that contain Cynewulf's runic signature in their closing lines, all in the Late West Saxon dialect, with only one copy of each extant. Two of these signed poems, Christ II and Juliana, are found in the Exeter Manuscript (ca. 950), while the other two, Elene and Fates of the Apostles, are located in the Vercelli Manuscript (ca. 950-1000). These four poems are the only ones in the Old English poetic corpus to which authorship is apparently given by the author himself, although authorial assignment was made for two other poems during the Old English period: Bede in his Historia Ecclesiastica Gentis Anglorum assigned authorship of Caedmon's Hymn to Caedmon in a vignette of the circumstances of its composition, and Bede's Death Song is ascribed to Bede in the manuscripts containing copies of it and written shortly after his death. For all the rest of the Old English poetry, authorship is unknown. Insofar as the poet Cynewulf is concerned, no hard historical evidence exists that establishes who he was, where he lived, what he did, and when he lived. Only the four runic signatures exist, and the lines containing the runes give only his name but no autobio-

graphical information. Because the signed poems all deal with religious topics, it is assumed by most scholars that Cynewulf was a monk. Although the name Cynewulf does occur in historical records, whether this Cynewulf is the poet Cynewulf or another Cynewulf altogether cannot be determined unless further historical evidence is found. However, the dates assigned in the literature for the poet Cynewulf are predicated on the dates of the historical Cynewulf (ca. 850-925).

The poems assigned to the Cynewulf Canon have varied throughout the years. The core of the Canon has remained those four poems containing the runic signature: Christ II, the Ascension; Elene, St. Helene's search for the true Cross; Juliana, a martyr for her faith; and Fates of the Apostles (afterwards given as Fates), an alliterative synopsis of the deaths and martyrdom of the twelve Apostles. Debate has occurred periodically over whether Fates should be included in the Canon or not, despite the runic signature. Some early investigators assigned other poems to the Canon, even though the poems lacked the signature, because of their similarity in content, style, poetic diction and tone: Judith, the very able, and in many ways quite terrifying, Jewish woman who slew Holofermus, thereby saving her purity and her people; Dream of the Rood, the impact of Christ's crucifixion on man, presented metaphorically as the impact upon the tree chosen to be the Cross; Andreas, the life of Saint Andrew; Phoenix, a pagan legend restructured in Christian terms to represent the resurrection of man; Christ I, the extremely moving Advent lyrics; Christ III, a typical presentation of Doomsday; and Guthlac A and B, prosaic accounts of the life and death of St. Guthlac. Today, only the signed poems are considered the true Canon, with the others placed in the Cynewulfian School, contemporaneous or shortly after the poet Cynewulf in time. The term 'Cynewulfian School' is somewhat misleading in that it suggests a school for poets dedicated to the composition of poetry in the style of Cynewulf. It actually means a body of poetry very similar to the style, tone, poetic diction, and topic found in the signed poems (cf. the "Introductions" and commentary on each edited poem in the Anglo-Saxon Poetic Records for the scholarship history of the individual poems of the Canon and the School, and the discussions in the various individual editions of the cited poems).

Actual determination of the true Canon has not been easy. Christ I and III, although unsigned, were included because they occur in the Exeter Manuscript along with Christ II, a signed poem, in the order I, II, III, all untitled, and so grouped by the manuscript scribe, apparently because of their topical continuity: Christ I, Advent; Christ II, Ascension; and Christ III, Doomsday. The manuscript order had a great influence on the inclusion of Christ I and III in the Canon. Today, it is agreed that they are not Cynewulf's work, not only because they lack the signature, but also because the three are not all that closely related in content - more a broad thematic relationship exists

than a specific one - with enough differences from the style and tone found in Christ II, Juliana, and Elene to deny Cynewulf's authorship. However, no really striking language differences among the three are discernable. Judith is now excluded because of the real language differences, some of which are easily noted, and because she is a Jewish heroine, not a Christian one as are Juliana and Elene. The original inclusion of Phoenix was because of its reworking in Christian terms of the pagan phoenix legend to explicate the resurrection of man and also to a certain extent on its style, tone, and poetic diction. Guthlac A and B, now thought to be two separate poems by two different poets (with B earlier than A in composition time), were placed in the Canon because both were concerned with an English saint's life. Dream of the Rood was originally included because of its religious focus and the occurrence of some of its lines in runes on the Ruthwell Cross (ca. late 8th Century) in spite of the quite perceptible language differences to the signed poems. In fact, today, there is a consensus that these seven poems do not belong to the Canon but to the Cynewulfian School. However, no complete linguistic analysis has been made to support this consensus so far.

A minor controversy still exists whether Fates, an apparently signed poem, belongs to the true Canon even with the signature, or whether Andreas, although unsigned, does. The manuscript order of Fates and its signature lines support its inclusion in the Canon, while the style and tone support its exclusion. The order of these two poems in the Vercelli Manuscript is Andreas followed by Fates. The lines containing the runic signature, considered by most scholars to belong to Fates (lines 97-122) stand alone on Folio 54a, while the first 95 lines are written on the preceding three folios ending at the bottom of Folio 53b with line 95. It is argued by some that Folio 54a is misplaced through scribal error and that it should follow Andreas. However, no manuscript evidence occurs to support this contention other than that the signature lines occur on a separate folio sheet. Andreas ends on Folio 52b, line 1722; Fates begins immediately below it on the same folio sheet, with its first line separated from the last line of Andreas by the usual Vercelli Manuscript spacing, a spacing identical to those found throughout the manuscript between different sections of any one poem and between the individual texts (cf. The Vercelli Book, 1932, xxxvii). The argument that the signature lines of Fates (96-122) should follow the end of Andreas (line 1722) is not supported by any physical evidence in the manuscript itself. Another, and somewhat better argument for assigning Andreas to the Canon is that Fates on aesthetic grounds seems a very unlikely candidate for canonical inclusion in spite of the runic signature. In style, tone, and poetic diction, Fates differs greatly from Christ II, Elene, and Juliana. This brief martyrology of the twelve Apostles has a style best characterized as doggeral. It is a very brief recitation of where the Apostles met their deaths and "who done it". No character development of either apostle or slayer occurs, and no motivation for their be-

havior is present; yet, both are skillfully developed in the other signed poems. All in all, Fates of the Apostles is just about the worst poem found in Old English and probably for all time. Andreas, however, is another matter. In style, tone, and poetic diction it seems to resemble the signed poems other than Fates. The development of its characters resembles closely that found in Juliana and Elene. In a way these three could be called "religious Beowulfs" without misrepresenting them.

Seven poems have been analyzed in this limited preliminary study: Christ I, II, and III, Juliana, Andreas, Fates of the Apostles, and Elene. The competing feature sets used to develop the preliminary profiles were those of negation, the demonstrative, the possessive pronoun, and case alternation. As this is a preliminary investigation, other feature sets originally thought to be relevant to the study were also used. However, this study has shown that they are not indicators, most probably because of the effect of poetic form (e.g., the word order of adjectives and genitive nouns in relation to the head noun; the placement of the possessive pronoun). Therefore, they have been dropped from the feature list for the complete analysis to be completed later.

This preliminary statistical investigation indicates support for both hypotheses. Christ II, Juliana, and Elene present language profiles consistent with being one author's work insofar as this restricted study is concerned. The profiles of Christ I and III suggest a different author for each, with neither Cynewulf. This study also suggests that the chronological order of the three Christ poems is III, II, and I (early to late) and with Christ II, Elene, and Juliana in the same time frame but not necessarily in this order (the preliminary study was not complete enough to determine the order of these latter three poems; it is also possible that their exact order may never be specifically determined as the study may not be finely enough tuned to determine order within a very restricted time frame). What is most interesting in this preliminary study is that neither Fates nor Andreas present profiles even slightly similar to those of Christ II, Juliana, and Elene. Each gives a profile closer to that of Christ III. This suggests that Andreas should not be included in the Canon to replace Fates and that Fates should not be included in the Canon either. If neither are Cynewulf's work, the possibility exists that lines 96-122 belong neither with Fates nor Andreas, but with another poem by Cynewulf, now missing. The manuscript organization suggests that if there is a missing poem, it was lost before the Vercelli scribe began writing the manuscript.

This investigation, insofar as it goes, has shown that the hypotheses may well solve the problem of which poems make up the Cynewulf Canon, which poems make up the Cynewulfian School, and which belong to neither the Canon nor the School. It has also shown that the methodology and techniques are valid, and that

a further complete analysis is warranted in order to develop complete profiles for each poem (using many more feature sets) to verify or deny the preliminary findings:

1. Juliana, Elene, and Christ II present very similar profiles which support their being the work of Cynewulf, the name given in runes ending each poem;
2. Christ I and III have authors other than Cynewulf;
3. Cynewulf wrote neither Fates of the Apostles nor Andreas;
4. The runic signature on Folio 54a indicates the possibility of a missing poem by Cynewulf; and
5. If Judith, Phoenix, Guthlac A and B, and Dream of the Rood do not present profiles consistent with those of Christ II, Juliana, and Elene, they do not belong in the Canon; whether they all belong in the Cynewulfian School remains to be determined.

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COMPUTERIZED ANALYSIS OF SYSTEMIC TREE DIAGRAMS
IN OLD ENGLISH

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Previous analysis of Old English according to the principles of Systemic linguistics has concentrated on the nominal group. This analysis proceeded from the hand-manipulation of data cards containing tree diagrams of nominal groups drawn largely from Old English homilies. The purpose of the present paper is to show the need for a computational method in extending this research, and to demonstrate the utility of programs written in the PROLOG language for the collating and retrieval of information recorded in tree-diagram form. In this computational system of analysis, data is entered in the form of tree diagrams representing Old English nominal groups. A PROLOG program converts this natural format into a more suitable, internal form. Grammatical questions seeking to identify individual trees or generalized features of trees are written as separate PROLOG subprograms and run on the data. Another PROLOG program automatically outputs answers given as whole nominal groups in tree-diagram form once again. The computational system is sufficiently flexible to be applied without radical modification to tree-diagram data describing any unit of grammar, including the sentence, at any degree of complexity, in any natural language.

Previously published work on the Systemic analysis of Old English nominal groups began with an analysis of groups and group complexes in the Old English homily Sermo Lupi ad Anglos. Analysis of this text yielded about 300 data cards, each bearing a hand-drawn tree diagram with extensions from the terminal nodes to designate word-class, case and text-word features. Hand-manipulation of these cards permitted generalizations about typical and atypical realizations of various structural elements in the groups and complexes, and afforded an orderly typology of nominal group structures in the homily (Cummings 1980). The same procedures were extended to include three other homilies, Bethurum 3, 5 and 21 (Bethurum 1957). The description of structural realizations and the structural typology was at that point based on approximately 600 nominal groups and complexes (Cummings 1981). A further extension of the description resorted to the account of nominal groups offered by traditional grammar in current manuals (Cummings 1982).

Description of the Old English nominal group which emerges

from these studies is based on the Systemic practice of applying functional labels to sequence-ordered slots. The modern English nominal group shows optional structural elements predeictic/deictic/postdeictic/numeralive/epithet/classifier before the non-optional head element, and optional qualifier element after head (Halliday and Hasan 1976: 39-43, 155, 159-161). Similarly the Old English nominal group shows optional premodifier elements, in this case determiner/epithet/nominal, and the optional qualifier element. Sequence-ordered determiner elements d_1, d_2, d_3 are analogous to the modern English predeictic, deictic, postdeictic and numeralive elements. The Old English nominal element occupies the same place as the modern English classifier element, but is much broader in function. Determiner elements are realized by deictic, numeral and quantifier words. The epithet element is realized by adjectives and participles, the nominal element typically by genitive or dative case nouns.

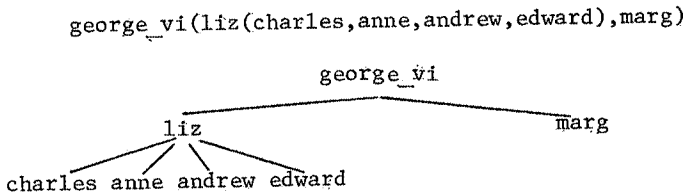
The qualifier element in Old English is frequently realized by rankshifted nominal groups and clauses; but it may also be realized by determiner words or adjectives in the same case as the head element, or by nouns in genitive or dative case, making the Old English nominal group much less distinctly polarized than its modern reflex. Rankshift may also occur at premodifier elements. Combinations of adverbial modifier and determiner-word or adjective-word realized head element constitute nominal groups rankshifted at either determiner or epithet elements. Various sorts of genitive or dative case nominal groups may be rankshifted at nominal element.

Tree structures for the Old English nominal group are complicated not only by rankshifts, but also by group complexes and word complexes just as in modern English. A series of two or more groups, usually contiguous, realizing the same element in clause structure, constitute a group-complex. Similarly a series of two or more words realizing the same element in group structure constitute a word-complex (Cummings 1980: 228-234; 1981: 348-353).

Hand-manipulation of data cards has proved satisfactory, even for a data base of 600 tree diagrams, in producing the syntactical analysis just sketched. For a larger data base, however, a computational method has seemed desirable. The data base which is currently under construction is a file of tree diagrams representing nominal groups and group-complexes in the entire corpus of Wulfstan homilies. This should comprise approximately 4000 tree diagrams. Many of the tree diagrams in this corpus will be of great simplicity, but many others will show multiple rankshifts to several degrees and long series of groups combined in complexes. Syntactical questions asked of this corpus involve distinguishing and instancing different elements, distinguishing and instancing many different possible combinations of elements in many different configurations, and selecting clear examples of patterns for exemplification. Each syntactical query could represent one pass through the entire corpus; this in itself suggests the utility of

machine analysis.

The general purpose computer language PROLOG was chosen as the basis of this analysis because PROLOG is particularly apt for programming analysis of tree structures.¹ PROLOG was developed in the mid-1970s as a tool for programming directly in logic. Its statements are made in a notation which is easily convertible into tree-structure data, and vice versa. The basis of PROLOG statements is called a "formula", which may be seen to have the pattern $X(Y, Z, \dots)$. In the formula pattern, Y and Z stand for character strings called "terms". The character string represented by X is called a "predicate-symbol". Within the formula's parentheses may occur any number of terms; if the number is zero, then the parentheses are dispensed with, implying that any simple character string itself falls within the definition of formula. Furthermore, each term within parentheses can be represented, not just by a character string, but by a formula-like structure, which permits an indefinitely large recursion.² Character strings are initiated either by lower case or by upper case characters. Lower case initiated terms are constants; upper case initiated terms are variables, standing for all constants or variables possessing the same notational environment elsewhere in the program or data file. (Pereira, Pereira and Warren 1978: 3-6; Baxter 1980: 10-15). The logic of formulae is the same as the logic of tree structures. Compare the following formula with its equivalent tree structure:



An elementary PROLOG analysis program presupposes a data file consisting of some number of formulae separated by periods, that is, distinguished as separate PROLOG "clauses", for example:

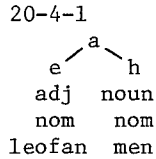
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a(b,c).
d(e,f,g,(h,i)).
c(j(k,l),m(n(o,p)),q).
b.
  
```

After this file has been loaded, PROLOG permits question programs to be entered interactively, after the prompt "?-". One such program might be "?- a(b,c)." and another might be "?- a(Y,Z)." The first of these programs effectively asks if the data file contains the formula "a(b,c)", and PROLOG responds "yes" in this case. The second program asks if the data file contains any formula having the general form "a(Y,Z)", where Y and Z are arbitrary terms. Not only does PROLOG respond "yes" in this case, but it also cites the first instance of terms from the data file

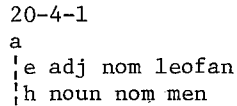
which satisfy these variables--"b" and "c" (Baxter 1980: 1-7). A question in the form "?- a(Y,Z), more." elicits from PROLOG all instances of terms which fit this pattern. Programs of this kind are called "pattern-match" programs.

The Old English nominal group data file shows a small notational variation from this basic procedure. Compare the following tree diagram with its equivalent formula:



ng(20-4-1,a:[e:adj/nom/leofan,h:noun/nom/men]).

The tree diagram is accompanied by an identification number for the nominal group, and contains word-class, case and text-word extensions from the terminal nodes. The data file formula embeds the tree proper in a larger bracketing which also contains the identification number. Hyphen, colon, square brackets and slant are notational variations which condense the nesting of round brackets into convenient infixes. These variations also help to distinguish different areas of the tree structure in programs. It should be noted that data is not keyed into file storage in bracket form, but in modified tree-diagram form, eg.:



This axially-rotated and mirror-inverted tree is much easier for a data entry clerk to interpret from the conventional tree of the data card than the bracket forms would be. The original data file is later rewritten in bracket form by a special PROLOG program.

The most elementary questions to be asked of the Old English data are of the pattern-match type. The question "What nominal groups are in the data?" can be asked interactively in the following form:

```

?- ng(I,T),
   write(I),
   draw(T),
   more.

```

The first line of this question program expresses the pattern for all nominal group formulae in the data, where I is a variable representing the identification number, and T is a variable

representing the second term of each formula, that is, the nominal group tree. The second and third lines of the program contain directives to write the number, and to output the nominal group in the original tree form. The "draw" directive requires prior consultation of a special PROLOG program.³ The last line of the question causes the question to be applied successively to all the trees in the file, and all trees to be successively drawn. A more sophisticated pattern-match question takes the following form:

```
?- ng(I,T),
    subtree(e:F,T),
    subtree(h:Class/Case/Text,e:F),
    write(I),
    draw(T),
    more.
```

The predicate-symbol "subtree", like the "draw" directive, is defined in a separate PROLOG program file. The first three lines of the question now express a pattern only for nominal groups containing a rankshifted nominal group at the e(pithet) element which has as its h(ead) element a terminal node. That is, line one expresses the pattern for all nominal groups in the file, line two delimits just those groups containing a subtree with e(pithet) as its vertex, and line three delimits the groups with a subtree of the previous subtree containing a h(ead) which immediately dominates word-class, case, and text-word extensions.⁴

PROLOG questions may also use a "set" predicate-symbol. Questions of this type do not retrieve tree diagrams. Rather they ask for a summary list of all the different constants represented by some variable in a particular notational environment. For example, a "set" question may ask for all the cases taken by words realizing the h(ead) element in all nominal groups in the data. This question takes the following form:

```
?-set(Case,I^T^Class^Text^
      (ng(I,T),subtree(h:Class/Case/Text,T)),
      Cases),
    write(Cases).
```

The answer to this question will be the list "[acc,det,gen,inst,nom]", for "set" also alphabetizes its answer lists.

The form of "set" question programs always falls into three parts. Line one of the above example starts with the "set" predicate-symbol which is defined in a separate file.⁵ The rest of the line names all the variables which will appear in the question, starting with the variable under scrutiny. The second line expresses a pattern for the nominal groups being asked the question, just as in a pattern-match. The third line expresses the plural of the variable under scrutiny, followed in the last line by a directive to write out this answer. All parts of the

"set" question take the form of ordinary PROLOG formulae, with much embedding of formula-like structures as terms.

Another very useful question program has much the same form as the "set" question except that the predicate-symbol "bag" is used in place of "set".⁶ Questions of this type count the number of instances in which some constant occurs in trees of a specified type. For example, we may wish to know the number of times a participle directly realizes the h(ead) element of a rankshifted nominal group realizing the e(pithet) element of some more inclusive nominal group. The question takes this form:

```
?- bag(I,T^F^Case^Text^
      (ng(I,T),subtree(e:F,T),
       subtree(h:part/Case/Text,e:F)),
      Is),
   length(Is,L),write(L).
```

Lines two and three of this question express the pattern of nominal groups under scrutiny. Note that the variable to be counted is the identification number of each nominal group; if the pattern occurs more than once in a nominal group, the group is counted each time. The "bag" question, unlike the "set" question, creates a list which is unsummarized and unordered. The directive "length" counts the list, expressed here as "Is".⁷ The number, expressed as "L" is then output by the "write" directive.

More complicated questions involve integrating or differentiating lists of nominal groups. In the following "set" question, the predicate-symbols "append" and "difference" (defined along with "member" in a separate PROLOG program) are used to add and subtract lists before a final syntactical query is made of the result:

```
?- set(I,T^F^F1^
      (ng(I,T),subtree(e:F,T),subtree(h:F1,e:F)),
      L1),
   set(I,T^F^F1^
      (ng(I,T),subtree(q:F,T),subtree(h:F1,q:F)),
      L2),
   append(L1,L2,L3),
   set(I,T^
      (ng(I,T)),
      L4),
   difference(L4,L3,L5),
   set(Class,I^T^F^Node^Case^Text^(member(I,L5),
      ng(I,T)subtree(e:F,T),subtree(Node:Class/Case/Text,e:F)),
      Classes),
   write (Classes).
```

This question creates a list (L1) of groups with rankshifts at e(pithet), a list (L2) of groups with rankshifts at q(ualifier),

and adds them together to produce L3. Then L3 is subtracted from the list of all groups (L4) to produce a list of groups purified of such rankshifts (L5). The final query asks for the list of word-classes which realize e(pithet) in this purified list of groups.⁸

Another predicate-symbol defined separately but extremely useful in question programs is "join". Unlike the predicate-symbol "subtree", "join" specifies in nominal group patterns not just a vertex of some subtree, but also two or more ordered nodes which it dominates, as in the following pattern-match:

```
?- ng(I,T),
    subtree(a:F,T),
    join([[N1:F1],[N2:F2],[h:F3],F4],F),
    write(I),
    draw(T),
    more.
```

This question seeks to output in tree-diagram form those nominal groups possessing two and only two premodifier elements.

The system of PROLOG retrieval subprograms is very flexible because patterns representing parts of or whole tree structures can be elaborated with great variety just by combining in various ways PROLOG formulae initiated with the terms "ng", "subtree", "member" and "join", and by representing the nodes in tree structures as either variables or constants in accord with the logic of the question. In major elaborations of the system (using some notation not discussed here), "set" question programs may be written to output lists of constants representing many variables at once, demonstrating all combinations which occur in the data. The forms of such answers are themselves tree structures, and can be regarded as tree diagrams which summarize the tree diagrams in the data. The system has the advantage that it is relatively easy to learn, since the user need learn techniques for writing subprograms which are much less elaborate than PROLOG itself. It should also be noted that character strings as terms representing nodes in tree structures are arbitrary from the viewpoint of the system, so that the tree structures can be descriptive of any rank in the syntax of any natural language.

FOOTNOTES

1. Throughout this paper "PROLOG" refers specifically to Version 3 of DECsystem-10 PROLOG (cf. Byrd, Pereira and Warren 1980). This version of PROLOG utilizes an interpreter with a built-in compiler, and a stand-alone compiler. All procedures sketched in this paper have been worked out and tested on DECsystem-10 using the interpreter and built-in compiler only.

2. When the formula pattern recurs as a term, its X is called a "function-symbol" and its terms may also be formula-like structures.

3. The character string "write" is a PROLOG directive in a proper sense, being already defined as an "evaluable predicate" in Version 3 of DECsystem-10 PROLOG (Pereira, Pereira and Warren 1978: 33), whereas "draw" is a predicate-symbol defined by the user in a separate PROLOG program which is consulted or compiled. A question in PROLOG can have any number of formulae separated by commas; each formula is a potential match for formulae in the data file.

4. The variable "F" represents everything dominated by the vertex node in the subtree.

5. It is based on the evaluable predicate "setof" (Byrd, Pereira and Warren 1980: 9-10) but designed to help manipulate empty sets. In general, the formula "setof(X,Y[^]P,S)" corresponds to the contents of the data if S is the collection of instances of X for which there exists Y satisfying P.

6. Like "set", "bag" is based on an evaluable predicate, "bagof", (Byrd, Pereira and Warren 1980: 10), but designed to help manipulate empty sets.

7. The "length" directive is an evaluable predicate (Pereira, Pereira and Warren 1978: 36).

8. Questions of this complexity are best not keyed interactively, but entered into file storage preceded by the notation "Q# :-" in which "Q#" may be taken to represent some identifying name. The question may then be asked simply by consulting or compiling the file, and keying in the name followed by a period.

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THE LANGUAGE-PROBLEM
IN MODERN ITALY

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It may well be a sociolinguistic "universal" that every speech-community in which one variety of usage is regarded as standard-setting has concomitant problems. (This may be simply one manifestation of a more general principle, that all standards bring problems with them.) In the matter of problems arising from the development and maintenance of a standard language, Italy has, for the last seven hundred years, led all the other countries of Europe. The traditional name for these problems has been, collectively, the *Questione della lingua*.¹

In the earlier stages of the debates, there were two main problems and a secondary one. The most important questions were: what variety of Italian should be the standard, and what stage in the development should be taken as a model for usage in later centuries? For various reasons, Tuscan gradually assumed this rôle between 1300 and 1500. Among these reasons were the linguistic conservatism of Tuscan (less changed from Latin than other Italian dialects); its central geographical position; the prestige of major authors such as Dante, Petrarch, and Boccaccio; and — last but far from least — the predominance of Tuscany in commercial and financial matters. It has been noted² that the use of Tuscan did not spread at an equal rate in all genres: it acquired prestige first in lyric poetry, then in other types of poetry, and last of all in prose.

Up to about 1500, the Tuscan which spread to non-Tuscan regions was simply the normal contemporary variety. By the sixteenth century, however, the language had changed sufficiently from that of the early fourteenth century for the difference to be self-evident. The problem then arose: in literature, especially, what kind of Tuscan shall we imitate, archaic or modern? A four-cornered debate took place, along the two axes of Tuscanism versus anti-Tuscanism, and modern usage versus that of the Trecento "Golden Age." So far as elegant literature was concerned, the "archaizing Tuscan" model was widely followed, through the teachings and prestige of the then dominant "vernacular humanism," embodied in the first part of the century in the work of the leading Petrarchist poet Cardinal Pietro Bembo, and in the latter part by the Florentine puristic critic Lionardo Salviati. A minor question, the name by which the language was to be called — should it be Florentine, Tuscan, or Italian? — caused much ink to be spilled but inevitably ended inconclusively.

The debates of the *Questione della lingua* were paralleled, and many of their arguments were echoed, in other European countries during the sixteenth and early seventeenth centuries.³ In France, Spain, and England, the language-problems were solved in a relatively short time, as a result of the political unification of the respective countries. In Germany⁴ and Italy, however, political unification was late in coming, and likewise the resolution of the language-problem. The desirability of a single language for the newly unified Italy was immediately recognized by the new central government, from 1860 onwards.⁵ In the three-quarters of a century from 1870 (the date of the final unification with the taking of Rome) to 1945, the official solution proposed for the *Questione della lingua* was that favored by the novelist Alessandro Manzoni (1785-1873), the uncompromising choice of current Tuscan and specifically Florentine usage as a universal standard.⁶

The *soluzione manzoniana*, as it came to be called, was opposed on theoretical grounds by a number of influential thinkers (including the linguist Graziadio Isaia Ascoli). In practice, the primacy of Tuscan was generally admitted, but in a somewhat diluted form, admitting the value and possible contributions of other dialects. The most influential proponent of this modified view was the popular, somewhat sentimental novelist Edmondo de Amicis (1846-1908). One might say that the *soluzione manzoniana* triumphed in theory, but that more of a compromise between the "pro-Tuscan" and "anti-Tuscan" view-points was reached in a *soluzione deamicisiana*.⁷

During the first half of the twentieth century it appeared, therefore, as if the *Questione della lingua* had reached its definitive solution and was a thing of the past. So it was, in the form which it had assumed from Dante's time down to 1900. Since 1945, however, other problems have arisen, constituting a new *questione*.⁸ Even after the extension of primary education, at least in name, to almost the entire population between 1920 and 1940, standard Italian remained the normal every-day language of only a relatively thin upper crust. For many — perhaps most — of the people, the standard was a second language, learned only in school and by no means always perfectly.⁹ There arose, consequently, regional varieties of the standard, with a fundamentally Tuscan base, but with peculiarities of phonology, morphosyntax, and lexicon carried over from the local dialectal substratum.¹⁰

This is a perfectly normal phenomenon, which has taken place many times in conjunction with the extension of a standard language to speakers of related dialects. What makes it different in Italy is that it has taken place so rapidly and in a country where the supposedly monolithic standard has achieved recognition so recently. Those brought up and taught in school to believe in a sharp opposition, an almost unbridgeable gap, between *lingua* and *dialetto*, and to regard the former as pure and the latter as inferior and impure, have been taken aback and have re-

acted with extensive discussion, much of it defensive and narrowly puristic.¹¹ More objective study has led scholars to distinguish between at least four, if not more, varieties of Italian to be found in any given locality: formal standard, informal (regional) standard, regional dialectal *coltā*, and local dialect.¹²

Increased public recognition of the existence and importance of dialects and regional standards has been greatly speeded, in the last twenty-five years or so, by sound-movies and especially by television.¹³ (That television has been much more influential in language-matters, world-wide, than radio is perhaps due to the combination of visual and auditory influences.) In both of these media, realism has demanded the use of authentic local varieties of speech, rather than an artificial language which no normal person would use.¹⁴

The new situation has brought new problems with it, involving policy with regard, not only to schools and the official teaching of the standard, but also to governmental and popular attitudes as expressed in legislation and in artistic matters. The existence and wide-spread use of regional standards can no longer be denied; to what extent should it be given recognition alongside of formal standard? On what city's or region's usage should the latter be based (Florence? Tuscany?¹⁵ Rome? Milan?)? How far should the authorities in charge of the national monopoly of radio and television (the *RAI-TV*) go in allowing regional accents to be used, not only in plays, but by announcers? Is Italian really the "literary" language any more, or should it be called, rather, the "national" language?¹⁶

All of these questions reflect the basic fact that language-problems and their solution now involve not only what goes on in *belles-lettres* and how the latter are taught in schools, but also real-life social and political matters. Discussion of the new *Questione della lingua* has moved out of learned circles into the forum of the public media. The relation between language and dialect, especially, is on its way to becoming a political foot-ball. In connection with a drive to establish regional sub-governments and to transform Italy from a centralized into a federal republic, recent publications¹⁷ have rather crudely attempted to create a persecution-complex by persuading dialect-speakers that they are victims of "linguistic imperialism" on the part of the educated classes, who are allegedly using the standard language as a means of oppression in reactionary class-war-fare. The standard itself, as taught in schools, is portrayed as devoid of vitality and inferior to living every-day dialectal speech. The defenders of Italian have responded with sarcastic portrayals of the disunity which would result from the abandonment of a common standard.¹⁸

Up to 1945, the concern of those who discussed language-problems was with the way to strengthen centripetal forces and thereby bring about linguistic unification. Now that this goal

has been brought in sight and in part reached, new forces, of a centrifugal nature, are at work attempting to split Italy again, linguistically as well as politically, by appealing to regionalistic and populist emotions. The out-come of the current discussions will of course depend on political decisions, but will also be affected by dominant attitudes towards centralization of communication and education. We may be allowed to hope that, although the old *Questione della lingua* served as a model and source for discussions and solutions of language-problems in other countries, the new *Questione* will not prove a harbinger of disunification elsewhere.

NOTES

1. There has been extensive discussion of the *Questione della lingua*, of the debates over it and of the gains in linguistics resulting therefrom, in the past eighty years. For over-all résumés, cf. especially Hall 1942; Migliorini 1949; Vitale 1978² (1960).
2. By Kristeller 1946.
3. Cf. Hall 1974:142-144.
4. In that country, German did not achieve wide-spread recognition as a prestigious literary language until the latter part of the eighteenth century; cf. Blackall 1959.
5. For an over-all discussion, cf de Mauro 1970² (1963).
6. Cf. such discussions as those of Migliorini 1960:684-69) and Migliorini-Griffith 1966:413-417; Monterosso 1971; Marazzini 1976; Peirone 1977.
7. Cf. Hall, forthcoming.
8. Cf. Parlangèli 1959.
9. Cf. de Mauro 1970²:36-45.
10. There has been extensive discussion of *l'italiano regionale*; cf. especially de Mauro 1970²:159-186, and the material listed in Hall 1980:\$§10780-10855.
11. As exemplified in extreme form in Fochi 1966.
12. Cf. Pellegrini 1974.
13. Cf. the (rather biased) discussion by de Mauro 1958.
14. Cf. the perceptive observations of the late Professor Carlo Battisti (1952), based largely on his experiences when enacting the title-rôle in Vittorio de Sica's film *Umberto D.* (1951).
15. It has been pointed out (e.g. by Folena 1958:135) that standard Italian is now undergoing a process of "de-Tuscanization" (*stoscanizzazione*) through the ever-increasing admission of non-Tuscan elements.
16. Especially when avant-garde littérateurs make the violation of the normal patterns of the standard language into a cardinal feature of their writings — a process which has been going on ever since the time of Filippo Marinetti (1876-1944) and the "Futurists" of the immediate pre-1914 period.
17. E.g. Salvi 1975 (cf. Hall 1978); Simonini 1976; Sobrero 1978.
18. E.g. Bolelli 1976.

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THE KENSINGTON STONE AS A FOLKLORE OBJECT

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Students with an interest in language and history, especially Middle Westerners, have been fascinated by the rune stone that was dug up in 1898 in west central Minnesota near the village of Kensington. I was born in southern Minnesota, not far from where the stone was unearthed; and although I soon moved to Iowa, the adjoining state to the south, and grew up there, I have early memories of feeling a regional pride that our cultural beginnings might possibly be traced back to 1362. I had merely English ancestry, and I can imagine how regional pride would be magnified if I had been of Scandinavian ancestry.

I wish to state here at the beginning that I have no competence whatsoever to discuss the question of the linguistic genuineness of the stone. What interests me most are other aspects. I feel that all linguistic evidence -- all use of language -- all speaking -- is inseparable from the social setting in which it originated and flourished. The stone is best studied, I believe, in a framework of folklore. By "folklore," I mean the oral transmission of attitudes and beliefs, although the documentation may come from newspapers, letters, and reports.

But questions have arisen, and whirlwinds of controversy have swirled around the stone with regard to its authenticity. Was it actually buried in 1362, in what is now west central Minnesota by Viking explorers, or was it concocted as a hoax, a prank, in the latter years of the 19th century?

For the linguist, the over-riding problem is the

authenticity of the text. Does the runic writing, as carved on a 200-lb. slab of stone, validly represent 14th century Norwegian-Swedish speech? Over the years since 1898 the answers to this question have see-sawed back and forth, and they have become specially urgent in the present year, when an esteemed member of our Forum, Robert A. Hall, Jr., has brought out his book with the provocative title, The Kensington Rune-Stone Is Genuine, issued by the Forum's reputable publisher, the Hornbeam Press. But I had planned to give this paper well before I learned of Hall's work.

The reports of the finding of the stone caused a flurry of interest locally in its own county, as well as further afield. In the early months of 1899, it was much reported as a sensational event in the newspapers of Minneapolis and Chicago. One of the earliest reports appeared in a newspaper of this town where we are now holding our Forum. The stone itself had been sent to Professor George O. Curme of Northwestern University for study, and the following item appeared in the Northwestern, a newspaper of Evanston, in the issue of March 9, 1899:

Last week the Runic stone, much heralded by the Chicago papers, came into the hands of Professor Curme of the German department. ... Professor Curme is well versed in the Runic tongue, but to make sure of the authenticity of the tablet he has sent a copy of the inscription to Adolph Noreen of Uppsala University, Sweden, the greatest living authority on the Runic language. Runic stones are very common in Norway and Sweden, but this is the first one ever found in America. The first thought is to consider it a hoax and to treat it accordingly, yet if proved authentic, this stone will change the whole history of America. Therefore, Professor Curme is justified, as a seeker of truth, to disregard the smiles of the uninitiated and to use every power at a scholar's command to prove the sincerity of the Runic stone. ¹

The interest subsided for a few years, and the stone was returned to its finder, the farmer Olof Ohman, and was laid down as a stepping stone to his granary. But then in 1907 the issue was revived when an independent historian, Hjalmar R. Holand, came to Douglas County to investigate it and became the lifelong champion of its authenticity. He was a dedicated crusader, even a zealot, and many people believe that the continued attention to the stone springs from his

advocacy, in many articles in newspapers and magazines, and in a series of books, till his death in 1963.

The museum committee of the Minnesota Historical Society brought in a report favorable to the Stone on May 9, 1910, but the Executive Council of the Society did not endorse it, for they had already learned that important scholars in Germanics and in runology held that the writing was a modern invention, not medieval.

One of the curiosities of the investigation is that the reputable scholar Frederick Jackson Turner was approached, but his report in a long letter of February 10, 1910, was not paid attention to and was virtually suppressed. He had produced the seminal paper in 1893, "The Significance of the Frontier in American History,"¹ and his rigorous recommendations for a proper procedure would have facilitated the investigation tremendously.²

Turner was aware of the social background and ethnic biases that we must take into account. As he said in 1910:

It is suspicious that the stone was found just about the right time after a general and heated discussion of whether Scandinavians were within the limits of the United States prior to Columbus; and in a Scandinavian community; but the puzzle should be worked out deliberately and not on the basis of assumptions one way or the other.³

The background of ethnic influence is well illustrated by the writing of Eric Sevareid, now the nationally famous journalist. At the age of eighteen, as a Dakota boy of Scandinavian ancestry, he accomplished the daring adventure in 1930 of canoeing with a companion all the way from St. Paul to Hudson's Bay. In his book of 1935, Canoeing with the Cree, he referred to the Kensington Stone in these words:

If the stone is a true relic of the fourteenth century, and most students of Viking history now believe it is, then white men visited the interior of America one hundred and thirty years before Columbus sighted its eastern coast -- and, it is quite possible that these Vikings had sailed into Hudson Bay, gone ... down Lake Winnipeg and into the Red River. Perhaps Walter and I had retraced the exact route on which the first European explorers had penetrated America.⁴

Roman Catholic support for the stone was strong, and in 1909 Archbishop John Ireland proclaimed that the stone added three centuries to the Catholic history of Minnesota.⁵ In 1952 a historian of the Catholic Church in Minnesota noted that the stone "gives our state a place in the sun peculiarly its own."⁶

As I said in a recent study of the intrusion of occultism into general semantics: "Every intelligent person should try to have a low 'gullibility index'."⁷ There seems to be a high gullibility index in the American populace at large. The eagerness with which ordinary people accept Viking origins is illustrated in the neighboring state of Iowa. The state archaeologist of Iowa, Duane C. Anderson, has said: "Vikings probably never visited Iowa, but many people believe they did." He goes on to remark that he "has seen rusty tobacco-plug cutters and Knights-of-Pythias swords passing as Viking artifacts. Someone did a newspaper story about a rock with a hole in it supposedly used to moor Viking longboats. Now every time someone finds a rock with a hole, it is a Viking rock."⁸

Even people with scholarly proclivities have shown this same tendency. One has only to think of Barry Fell's contention that the Celts came to New England many centuries before the Christian era and left Gaelic roots that now appear in the Indian place names of Vermont and other New England states.⁹ This has been amply refuted by W. F. H. Nicolaisen in his study, "Celtic Place Names in America B. C.,"¹⁰ but Barry Fell remains unfazed.

An institution at Port Jervis, New York, called the Middletown Archeological Research Center, is devoted to finding "stone structures, monoliths, artifacts and etched stones of Northeastern United States" which "suggest possible connection with Celtic, Balearic and Phoenician cultures of three thousand years ago."¹¹ They use aerial surveys, with high-altitude infra-red photography, to expose ground disturbances contrary to colonial and contemporary landscapes. Also Cyrus Gordon, of New York University, according to his Riddles in History (1974), finds Canaanite inscriptions in Brazil.¹² This syndrome of a willingness to accept the wondrous has a bearing on the evaluation of the Kensington Stone.

For members of this Forum especially, we must revert to the subject of runological authenticity. As I have already said, I have no expertise whatsoever in

this field, but I have explored the Kensingtonian literature with fascination. The earliest scholar consulted, O. J. Breda, Professor of the Scandinavian Languages at the University of Minnesota, translated the inscription in 1899, but decided against its genuineness, and Frederick F. Klaeber was not even approached. George O. Curme tackled the Stone with an open mind but decided in the negative and was supported almost to a man by the runologists of Norway and Sweden. My own early teachers and their contemporaries, namely Kemp Malone, Henning Larson, Sir William Craigie, Chester N. Gould, were also negative. George T. Flom was a persistent adverse critic of the Stone.

It came as a surprise in July, 1950, when the prestigious journal Speculum, organ of the Medieval Academy of America, published a long defense of the Stone by Sivert N. Hagen, going over the details item by item,¹³ but in 1958 Erik Wahlgren's book The Kensington Stone: a Mystery Solved seemed to give a coup de grâce to all defense of the Stone. This point of view was furthered in 1968 by the appearance of the book of Theodore C. Blegen, The Kensington Rune Stone: New Light on an Old Riddle. Blegen, as the most eminent of regional historians, brought his meticulous scholarship to bear on all aspects of the problem.

It has now come as a bombshell that a fellow-member of our Forum, Bob Hall, has in the present year brought out his book, The Kensington Rune-Stone Is Genuine.¹⁴ Its author has unassailable standing as a careful scholar, and the book can be studied as a guide to methodology in such investigation. I would be willing to go so far as to say personally that I "revere" Bob Hall, for his courageous defense of us embattled usage people in departments of English, set forth in his Leave Your Language Alone, which I have used as a text in my own courses, and for his stalwart defense of sound linguistics in recent years against the ideological intrusions that would undermine linguistics as a science.

Hall does not have the ethnic bias that I have mentioned, but he does have a bias that is indicated by the epithet that he chose for the title of his autobiography, "the stormy petrel."¹⁵ He is prone to taking the difficult, contentious side in any dispute.

But even Bob Hall's judgment must be subjected to severe scrutiny. This is necessary because of a development that has taken place within the past six years. Back in 1968 Theodore Blegen said in the final para-

graph of his book:

New sources, as yet unused, will turn up; there will be further discoveries. I write these lines with confidence, for I am aware of tape recordings of interviews with two persons qualified to throw new light on the origin of the Kensington inscription as modern and local. These are in private safekeeping and are not available. I have not heard them and do not know their contents. I do know that ultimately they will be placed in the Minnesota Historical Society. 16

At last, in 1976, the tapes that Blegen referred to were released and a transcription made available in the journal Minnesota History. 17 They record the information of John P. Gran, the farmer who lived next door to Olof Ohman. They were taken down as part of an oral history project, from Gran's son and daughter in 1967 and from his nephew in 1970, setting forth what was common knowledge in their family, often discussed. They sound to me so authentic that I regard their information as unimpeachable. In recent years the phrase has become current, "the smoking gun"; and I believe that in these tapes we have the smoking gun that makes a belief in the Kensington Stone no longer tenable.

They reveal that John Gran and Olof Ohman, as fellow carpenters who built houses in the neighborhood, collaborated in the concoction of the inscription and the incising of it. Ohman's oldest son got tired sharpening the chisels on a hand-turned grindstone. The complicity of the well-educated Sven Fogelblad was also made clear.

Of special importance, the motivation is supplied. John Gran's son reports a conversation between his father and Ohman, in which Ohman says: "... wouldn't it be nice to make some scripts that would bluff the people in this here territory, and them educated guys. Yeah, Dad says, that would be something. So they agreed on that right there." 18

I do not have time to quote further, but I assure you that the tapes carry conviction.

Writing in 1982, Bob Hall was aware of the Gran tapes, but he denigrated them as "loose talk" and called them "gossip-stories." 19 Certainly much gossip had circulated in the neighborhood, as Blegen

and others have pointed out, but John Gran's material is not of that nature. Bob Hall's "gullibility index" has its ups and downs. He is severe in his treatment of John Gran, but is willing to accept the stories that Ohman told about the circumstances of finding the stone. Bob Hall has demonstrated two things: (1) that the forger or forgers were cleverer than might be expected, and (2) that the parameters of runic writing are broad enough to accommodate a modern imitation.

The question has often been asked, how could a hoax have been committed without the neighbors knowing about it. The answer seems to be that the neighbors did know about it and simply played along, with varying degrees of good humor.

We can re-fashion an old adage by saying, "If the Kensington Stone did not exist, it would be necessary to invent it." That may well be what actually happened. The desire for historical roots, among Scandinavians of the upper Middle West, was so strong that the Kensington Stone was fastened upon to answer this need. As Finar Haugen wrote in a personal letter to me: "the Stone is valuable as a focus of folklore, or even more widely, as a testimonial of immigrant authenticity."²⁰ It is in this light, even apart from runological considerations, that we can find deep interest in the Kensington Stone.

NOTES

1

Quoted by Theodore Blegen, The Kensington Rune Stone: New Light on an Old Riddle (St. Paul, 1968), p. 132.

2

The letter was printed and discussed by Theodore Blegen, "Frederick J. Turner and the Kensington Puzzle," in Minnesota History, Vol. 39 (Winter, 1964), 133-40.

3

Op. cit., p. 140.

4

Eric Severeid, Canoeing with the Cree (New York: Macmillan, 1935), p. 187.

5

Cited by Erik Wahlgren, The Kensington Stone, a Mystery Solved (Madison, Wis., 1958), p. 183.

6

James Michael Reardon, The Catholic Church in the Diocese of St. Paul from Earliest Origin to Centennial Achievement (St. Paul, 1952), as quoted by Wahlgren, p. 183.

- 7 Not Just Words: the Newsletter of Transpersonal Linguistics, Vol. I, No. 4 (Spring, 1982), p. 8.
- 8 University of Iowa Spectator, Vol. 9, No. 6 (May, 1976), p. 8, col. 3.
- 9 Barry Fell, America B. C.: Ancient Settlers in the New World (New York: Quadrangle, the New York Times Book Co., 1976), pp. 5-7, and especially Chapter 16, "New England's Celtic Place Names," pp. 247-51.
- 10 W. F. H. Nicolaisen, "Celtic Place Names in America B. C.," in Vermont History, Vol. 47 (Spring, 1979), pp. 148-60.
- 11 From its publicity handout, 1977. For a full exposition, see Salvatore Michael Trento, The Search for Lost America: the Mysteries of the Stone Ruins (Chicago: Contemporary Books, 1978), xxiv, 284 pp. As the author said, p. xviii: "The point is obvious: seek and ye shall find."
- 12 Cyrus H. Gordon, Riddles in History (New York: Crown Publishers, 1974), pp. 71-92.
- 13 Sivert N. Hagen, "The Kensington Runic Inscription," in Speculum: a Journal of Mediaeval Studies, Vol. 24 (July, 1950), 321-56. For Wahlgren and Blegen, see footnotes 5 and 1 above.
- 14 Robert A. Hall, Jr., The Kensington Rune-Stone Is Genuine: Linguistic, Practical, Methodological Considerations (Columbia, S. C.: Hornbeam Press, 1982), xii, 109 pp. His interest in the subject is of long standing, as he produced an article defending authenticity in September, 1950, printed as an appendix to the new book, pp. 99-104. See also his excellent summary of the whole problem, "The Philological Debates over the Kensington Rune-Stone," in the Festschrift for the Chilean scholar, Ambrosio Rabanales, to appear soon. Because of the delay in its publication, I have not seen Hall's contribution to The Eighth LACUS Forum 1981, "Linguistic Mixture in the Kensington Runic Inscription."
- 15 Robert A. Hall, Jr., Stormy Petrel in Linguistics (Ithaca, N. Y.: Spoken Language Services, 1975), vi, 230 pp.
- 16 Blegen, 1968, p. 124.

- 17 Russell W. Fridley, "Debate Continues Over Kensington Rune Stone," in Minnesota History, Vol. 45 (Winter, 1976), 148-51. Also "The Case of the Gran Tapes: Further Evidence on the Rune Stone Riddle," ibid., pp. 152-56.
- 18 Op. cit., p. 156.
- 19 Robert A. Hall, Jr., 1982, pp. 54-55.
- 20 Letter of May 29, 1982. Haugen's interesting contributions to the problem are contained in a letter of March 7, 1968, now in the Kensington file, Blegen Papers, Minnesota Historical Society, referred to in Blegen's book of 1968, p. 123, note.

ARE PERSONAL-NAME-DERIVED PLACE NAMES SEXIST?

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There is a commonly accepted claim, now part of the folklore associated with any discussion of sexism in language, that place names derived from the personal names of individuals are based on the surnames of males but on the first names of females (e.g. Nilsen 1980:7). The usual explanation given for this difference is that "traditionally the given name has been the one that stayed with a woman her whole life, while her family name changed when she married" (Nilsen 1980:7), whereas for a man, his family name stays with him for his whole life. It is my purpose in this paper to show that this claim is incorrect and that there is a simpler explanation for the choice between first name and surname which correctly accounts for a much wider set of place names.¹

Let us refer to this commonly accepted claim as *Hypothesis 1*: "*Place names derived from the personal names of individuals are based on the surnames of males but on the first names of females.*" A cursory look at almost any map will reveal that Hypothesis 1 fails badly with respect to two categories of personal-name-derived place names: names involving saints and names involving royalty. For example, there are place names derived from the first names of male saints (such as San Antonio, St. John's, St. Augustine, St. Louis); a longer (but by no means exhaustive) list may be found in Table 1. There are also place names derived from the first names of male royalty (such as Georgetown, Alberta, the Carolinas, Franz Josef Land, Lake Edward); more may be found in Table 2. Such names are particularly common in parts of the world formerly ruled by the British or French Empires. Thus, Hypothesis 1 fails, but we can take account of these two categories of exceptions and revise our hypothesis to *Hypothesis 2*: "*Place names derived from the personal names of individuals are based on the surnames of males (except for saints and royalty), but on the first names of females, saints, and royalty.*"

Hypothesis 2 seems to account for the data so far, but a few nagging doubts led me to attempt to verify Hypothesis 2 by checking through the origins of all the place names in a medium-sized dictionary of place names. For this purpose, I selected Room 1974, which contains approximately 1800 place names from around the world. Approximately 350 of these place names were derived from personal names, and, of these, 6 were exceptions to Hypothesis 2. For completeness, I list the exceptions here: Charleston (W.Va.) is named after the first name of the father of the founder of the town, George Clendenin; Jamestown (N.Y.) is named after James Prendergast, an early settler; Johannesburg

(South Africa) has a disputed etymology, but, depending on which source is consulted, is named for Johannes Rissik and/or Johannes Meyer, both of whom were early farmers in the area; Ladysmith (South Africa, also British Columbia) is named for Lady Smith, wife of Sir Harry Smith, British Governor of Cape Colony from 1847 to 1852; Pierre (S.D.) is named for Pierre Choteau, an early fur trader; and Simonstown (South Africa) honors Simon van der Stel, Dutch Governor of Cape Colony from 1679 to 1699. For five of these, the exception involves a place name from a male derived from a *first* name (where royalty and saints are not involved); for Ladysmith, the exception involves a place name from a female derived from a *surname*.

Should we retain Hypothesis 2, but simply recognize that, although it can explain an overwhelming majority of personal-name-derived place names, there are a few exceptions? I think not, because there is yet more evidence against Hypothesis 2, although not from Room 1974. As in many disciplines, but particularly in linguistics, once the mind becomes sensitized to observe a particular phenomenon, examples and exceptions begin to pop up everywhere one looks. Not only are there plenty more of the types of exception to Hypothesis 2 found in Room 1974 (see Tables 3A and 3B for some examples), but there are also other types of exception. There are place names derived from the *full* names of individuals of either sex, such as Harrismith (husband of the Lady Smith above), Ho Chi Minh City (Vietnam), Mount Edith Cavell (Alberta), and such Newfoundland gems as Joe Batt's Arm. More are listed in Table 3C. There are place names derived from full "professional" titles, such as Ingeniero Jacobacci (Argentina), Comodoro Rivadavia (Argentina), Presidente Prudente (Brazil), and Princess Elizabeth Land (Antarctica). (Some readers may feel that Ladysmith should be included in this category). Strangely enough, almost all of these seem to occur in South America and the polar regions of exploration. Further examples can be found in Table 3D. Also, many place names involving surnames are not in fact named for any specific male of that family (e.g. Smiths Falls and Bolton, both in Ontario; see also Table 3E), but are in fact named for the family as a whole. Indeed, the only sexism involved here is in ourselves for so readily assuming that such names are derived from a male!

Hypothesis 2 clearly fails. But can any *other* hypothesis explain the choice between first name and surname for personal-name-derived place names? I believe that a very simple hypothesis, *Hypothesis 3*, can explain all of our data: "*Place names derive from the name by which the individual is most widely known in the relevant speech community.*" In addition to being simple and accounting for all the data correctly explained by Hypothesis 2, Hypothesis 3 has the merit of being able to explain the derivation of certain other similar types of place name, often *not* accounted for by the original claim (i.e. by Hypothesis 1 or its modification, Hypothesis 2). For example, there are place names derived from nicknames (e.g. Young Nick's Head (New Zealand), Little Pete Meadow (California); see also Table 3F), which reflect

the name by which the individual is known in the community. In Canada there are place names with the elements "Frenchman" or "Indian", which are essentially nicknames for individuals who in some sense stand out in a particular speech community because of their ethnic origin (e.g. Frenchman Butte (Saskatchewan)). H.A.Gleason, Jr. (personal communication, 1982), informs me that there are place names derived via community church names from individuals or families, particularly in the rural south of the United States. In the case of families, the surname is generally used and will usually represent the family that donated the land for the church. In the case of individuals (whether male or female), the first name is normally used, although occasionally the full name is used. Apparently the number of females so honored is approximately ten times the number of males for two reasons: first, there are more widows than widowers (i.e. who are able to devote considerable time and/or money to the church); second, a woman who dedicates herself to the church generally remains with the one parish, which may rename its church to honor her after her death, whereas a man who dedicates himself to the church normally is ordained and expands his sphere of interest to serve more than just the one parish. One obvious remaining type of place name which can be accounted for by Hypothesis 3 (but not by Hypothesis 2 as it stands) involves those from cultures which generally lack surnames (for example, Hannibal, Gibraltar, Hastings, and literally hundreds more British place names dating from pre-Norman times; further examples may be found in Table 3G). Here the first name is sometimes supplemented by a sobriquet or epithet, such as Jebel-al-Tarik (>Gibraltar).

Hypothesis 3, that place names derive from the name by which the individual is most widely known in the relevant speech community, can explain all the data examined. Thus the actual derivation of place names in itself is *not* sexist (*contra* claims such as that made by Nilsen 1980:7), although there might be sexism involved in the fact that (in our culture, at least) a man is generally most widely known by his surname whereas a woman is most widely known in her community by her first name.

Table 1: Place Names Derived from the First Names of Male Saints

San Antonio, San José, St. Petersburg (Fl.), St. John's, St. Augustine, St. Louis, San Francisco, San Luis Obispo, St. David's Head, St. Etienne, St. Francis, St. George, St. Gotthard Pass, St. Joseph, St. Lawrence River, St. Martin, St. Paul, St. Pierre, St. Stephens, Cape St. Vincent, St. Cloud, St. Denis, St. Bernard Pass, St. Kitts, St. Malo, St. Moritz, St. Tropez, San Marino, San Remo, Santiago, San Diego, São Paulo, San Bernardino, San Clemente, San Juan, San Juan Capistrano, San Sebastian, San Pedro.

Table 2: Place Names Derived from the First Names of Male Royalty

Lake Albert, Lake Edward, Franz Josef Land, the Carolinas, Charleston (S.C.), Wilhelmshaven, Georgia, Christiania (now Oslo), George Town, Georgetown, Jamestown (Va.), Petrograd, Karlovy Vary/Karlsbad, Karlsruhe, Mauritius, Monte Carlo, Alberta, Albertville (Congo), Léopoldville (Congo), Caroline Islands.

Table 3: Other Types of Exceptional Place Names²

A: Derived from First Names of Males other than Saints and Royalty

Guysborough (N.S.), Fort William (now Thunder Bay, Ont.), Jasper (Alta.), Peterborough (Ont.), Pierre (S.D.), Simons Bay (South Africa), Simonstown (South Africa), Arthur's Pass (New Zealand), Logan (Neb.), Williamsport (Pa.), Danville (Va.), Petersburg (Va.), Ferdinandina (Fl.), Eugene (Ore.), Gene (Okla.), Charleston (W.Va.), Jamestown (N.Y.), Johannesburg (South Africa), Nick (Ky.), Charles (Ky.), Phil (Ky.), Jacques (Mich.), Bud (Tenn.), Willis Hole (Calif.), Tom (Tenn.), Port Arthur (Texas).

B: Derived from Surnames of Females

Ladysmith (South Africa, B.C.), Ladybrand (South Africa), Cape van Diemen (New Zealand), Bancroft (Ont.).

C: Derived from Full Names of Males or Females

Juan de Fuca Strait (B.C.-Wash.), Juan Fernandez Island (S. Pacific), Juan Perez Sound (B.C.), James Ellsworth Land (Ant.), Mount Edith Cavell (Alta.), Dolly Varden Lake (Minn.), Mount Fridtjof Nansen (Ant.), Marie Byrd Land (Ant.), Dirk Hartog's Island (Australia), Trislan da Cunha (S. Atlantic), Joe Batt's Arm (Nfld.), Harrismith (South Africa), Louis Trichart (South Africa), Itmann (W. Va.), Mabowden (Man.), Piet Potgietersrust (South Africa), William Henry Marsh (Ont.), Diego Ramirez Island (Chile), Jan Mayen Island (Arctic), José de San Martín (Argentina), Teófilo Otoní (Brazil), Vila Henrique de Carvalho (Angola), Vila de João Belo (Mozambique), Paulo Alfonso Falls (Brazil), Alcedale

Table 3C (continued)

(South Africa), Joaquin González (Argentina), João Pessoa (Brazil), Fernando Po (Equatorial Guinea), Isla Fernando de Noronha (Brazil), Luís Corneira (Brazil), Benjamin Constant (Brazil), Adamana (Ariz.), Antross (Canada), Ho Chi Minh City (Vietnam), Lourenço Marques (now Maputo, Mozambique), Amund Ringes Island (N.W.T.), Antonio Enes (Mozambique), Cape Leopold McClintock (N.W.T.).

D: Derived from Full "Professional" Titles

Lord Howe Island (Australia), Prince Albert (Sask.), Prince Edward Island (Canada), Prince Rupert Land (Canada), Ingeniero Jacobacci (Argentina), Comodoro Rivadavia (Argentina), Gobernador Gregores (Argentina), Libertador General San Martín (Argentina), General Pico (Argentina), Presidente Roque (Argentina), General Alvear (Argentina), Coronel Pringles (Argentina), General Lavalle (Argentina), Comandante Luis Piedrabuena (Argentina), Fray Bentos (Uruguay), Mariscal Estigarribia (Paraguay), Presidente Prudente (Brazil), Governador Valadares (Brazil), Ferreiro Gomes (Brazil), Senhor de Bonfim (Brazil), Queen Charlotte Islands (B.C.), Dronning Maud Land (Ant.), Princess Elizabeth Land (Ant.), George V Land (Ant.), King Christian Island (N.W.T.), King Christian IX Land (Greenland), King Edward VII Land (Ant.), King Frederick VI Land (Greenland), King George Island (S. Shetlands), King George Mountain (B.C.), King George Sound (Australia), King Leopold Range (Australia), King William Island (N.W.T.), Queen Adelaide Archipelago (Chile), Queen Alexandra Range (Ant.), Queen Maud Gulf (N.W.T.), Lady Ann Strait (N.W.T.), Lady Elliot Island (Queensland), Lady Evelyn Falls (N.W.T.), Lady Newnes Bay (Ant.), Presidente Epitacio (Brazil), Princess Astrid Land (Ant.), Prince Gustaf Adolf Sea (N.W.T.).

3E: Derived from Surname of Family

Bolton (Ont.), Smiths Falls (Ont.), Kingston Lacy (Britain), Melton Mowbray (Britain), Acton Turville (Britain), Glamorgan (Britain), Brockton (Mass.), Lawrence (Mass.), Lewiston (Maine).

3F: Derived from Nicknames

Young Nick's Head (New Zealand), Little Pete Meadow (California), Frenchman Butte (Sask.).

3G: In Cultures Lacking Surnames³

Barking (England), Worthing (England), Godalming (England), Reading (England), Hastings (England), Basingstoke (England), Birmingham (England), Paddington (England), Kensington (England), Ipswich

Table 36 (continued)

(England), Woking (England), Washington (England), Coventry (England), Dagenham (England), Kenilworth (from female name, England), Wolverhampton (from female name, England), Bambergh (from female name, England), Bognor (from female name, England), Tübingen (Germany), Esslingen (Germany), Singen (Germany), Solingen (Germany), Mannheim (Germany), Sigmaringen (Germany), Hyannisport (Mass.), Nonesi's Nek (South Africa), Modjadjisberg (South Africa), Kabalega Falls (Uganda), Maracaibo (Venezuela).

FOOTNOTES

¹I am specifically considering place names derived from personal names. Thus I am omitting place names derived from "offices" which individuals may hold (e.g. Queenston, Kingston, Admiral Islands, Queensland, Knightsbridge, Princeton, Port-au-Prince, Prince of Wales Island, etc.). I am also omitting place names derived from the names of mythical founders and gods, for the practical reason that many of these names are ancient and disputed. Such names, incidentally, pose no problem for Hypothesis 3, although they do pose problems for Hypotheses 1 and 2.

²It is obviously impractical to give a source for each etymology individually. The major reference works consulted were: McConnell 1978, Matthews 1972, Raper 1972, von Engeln and Urquhart 1970, Lambert and Pei 1959, and Stewart 1975.

³Some of these are named after tribal leaders. Thus some readers may feel that these should properly be classified as "royalty". The conclusions of this paper would not thereby be changed.

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ISBN 0-917496-23-X



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